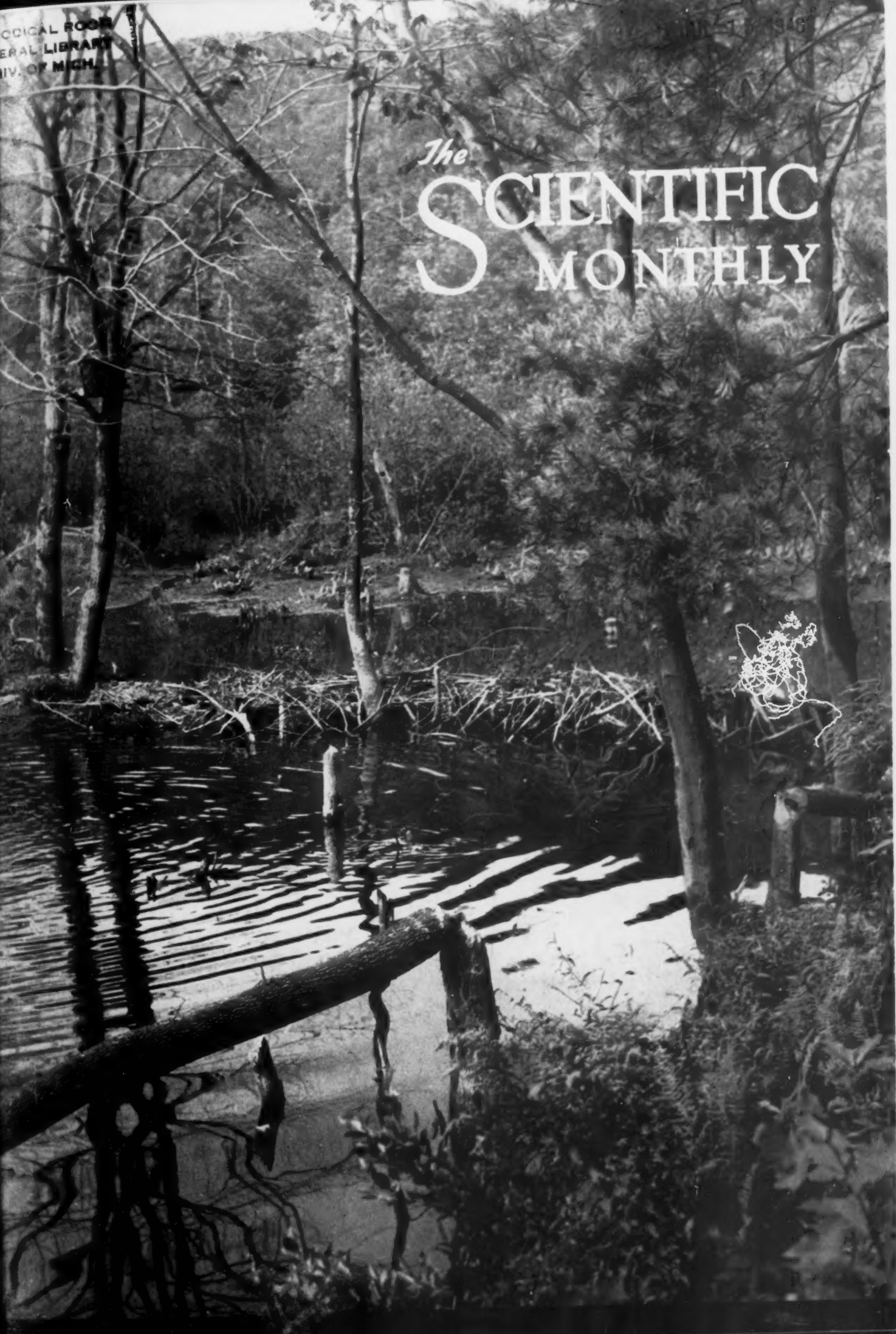
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THE SCIENTIFIC MONTHLY

AUGUST 1948

THE MAGNIFICENT RODENT*

H. RAYMOND GREGG

Mr. Gregg, a graduate of Hendrix College, Conway, Arkansas, is chief of the Naturalist Division, National Capital Parks. He has made many contributions to the field of nature education, including the Junior Nature School, Rocky Mountain National Park, one of the pioneer efforts in outdoor education for juveniles. His "Nature Sketches" broadcast from the Park were an NBC network feature for six years prior to his transfer to Washington.

AS RECENTLY as forty years ago, when few people had the opportunity to visit the remote remnants of wilderness in our country, it was only an occasional person who had seen a beaver in the wild, or observed its works. The once-fashionable gentleman's beaver hat had become a collector's item, and a fine beaver coat for milady was a faint social will-o'-the-wisp. To most people, the beaver had become largely a legendary symbol of industry. Yet, in the America of our fore fathers, this oversized rodent inhabited the whole continent north of Mexico, except the riverless wastes of the Great Basin, the frigid Arctic tundras, and the extreme southeastern coastal plain.

The sheer weight of aggression by a people who cleared the land, polluted streams and diverted their waters, and exercised strong intolerance of wilderness neighbors inevitably must have reduced greatly the range and population of beavers. But it was not slow pressure of displacement that brought the beaver to its sorry plight in 1900. A hundred years of persistent trapping for the market dangerously lowered populations while vast acreages of habitat suitable for beavers remained relatively unimpaired. Four phenomenal decades of expansion, exploitation, and social excitement between 1805 and 1845 brought feverish and far-flung activity by trappers and fur traders. By 1850, almost throughout the United States, beaver trapping had reached a point of diminishing returns, and the species approached or had reached extirpation over much of its former range.

There are published estimates of aboriginal population as high as 400,000,000, but 60,000,000 is a more acceptable figure. Such estimates necessarily are based upon limited data, and probabilities. However, on a comparable basis, possibly the 1900 beaver population for the United States proper could be covered in five figures.

The ground swell of conservation consciousness of the early 1900s brought attention to the beaver as a vanishing species. Writers, some of them more enthusiatic than accurate, stimulated widespread interest in the beaver and its way of life. By World War I, stringent or total protection was provided by law almost everywhere. Widespread efforts were begun to restock depleted territories, or even to extend the known range of beavers. The ensuing years have brought fruitful results. Today, relatively few Americans live more than a day's drive from a wild-beaver colony.

A 1940–41 survey of eleven Pacific Coast, Rocky Mountain, and Great Basin states reported a beaver population of 324,000. During that same year, removal of nuisance beavers in those states produced 53,936 pelts, worth $1,096,659. Also in that year,

* Photograph on page 78 by Paul Nesbit. All others by National Park Service.

even in the former "dust-bowl" state of Kansas, 368 beavers were trapped. New Mexico and North Dakota, unassociated in popular thinking with beavers, harvested 1,160 and 1,800 beavers, respectively. The year before, 72 were taken in Texas! Restoration of beavers has not been in the West only. In Maine, 3,049 beavers were taken during 1940–41; 1,023 in New Hampshire; and 1,195 in Pennsylvania. At the Sixth North American Wildlife Conference in 1941, Miss Fannye A. Cook, of Mississippi, reported removal of beavers to placate complainants in that state. Virginia was progressing with re-establishment of beavers.

Table 1 presents data for 1946 from states that responded to a questionnaire. Since the early 1930s, Michigan and Wisconsin have developed a sustained-yield fur resource and have compiled a wealth of information basic to sound management. G. W. Bradt (*Michigan Beaver Management*, Michigan Conservation Commission, 1947) and N. R. Barger (A Peak in Wisconsin's Beaver Harvest, *Wis. Conservation Bull.*, November 1947) have described the programs in those states.

In Michigan, 15,296 beavers were trapped in 1945. In 1946, it was still possible to harvest 9,859 pelts, although the season was closed in 1947 as a precaution against overdrawing breeding stock. Price stimulus (the 1946 take, at an average of $40 per pelt, grossed $394,360) may have induced harvesting beyond the productive capacity of the population. However, in seven years, 1938–44, with the Upper Peninsula closed in 1942 and 1944, there was an average annual yield of 5,663 pelts. It is likely that improving management and trapping practices will bring consistently higher yields.

Between 1936 and 1943, Wisconsin averaged 4,571 beaver pelts, including a zero figure for the closed season of 1940. The annual harvest varied in direct proportion to pelt prices. This was most striking when the 1944 figure jumped to 7,720 from a total of 4,564 in 1943. The season was closed in 1945. Then, in 1946, a harvest of 15,280 beavers was taken, bringing gross revenue of $702,880!

As a final statistical tribute to the capacity of beavers to thrive when aided by man, consider the case of Montana, where 10,200 pelts were taken in 1946, even after peak harvests of 12,752 in 1940; 17,717 in 1941 (a modern record for any state); and 11,161 in 1942.

Probably two factors contribute most to the beaver's capacity for survival and recovery: adaptation to life in an aquatic environment, and partially concomitant relative ineffectiveness of predators other than man.

Under conditions favorable to the predator, most of the land carnivores can kill a beaver. Probably wolverine, cougar, lynx, bobcat, coyote, and wolf have been most effective. Fisher, otter, and even mink, all capable swimmers, are proved or probable predators, but the extent of their predations is unknown. The primitive abundance of beavers, when predators also were abundant, indicates good native ecological adjustment.

The mature beaver is a formidable adversary when attacked. Even man is not immune (although game wardens are not taken in by trappers, caught with illegal pelts, who plead self-defense). The beaver has strong, sharp incisor teeth and powerful jaws. The body is stocky, thick-necked, tough-skinned, with low center of gravity—all excellent defensive traits. A record California Golden Beaver exceeded 80 pounds. A male, drowned in live-trapping operations in Rocky Mountain National Park, weighed 55 pounds; it was not unusually large for that region. Accidents such as crushing by falling trees, internecine warfare, and internal parasites probably account for more beavers than predation.

Adaptable to a great variety of aquatic and wet-association food plants, and tolerant of great variations in temperature, the beaver had one of the greatest aboriginal territorial ranges among mammals of this continent. Southern sloughs, rivers penetrating desert lands, and cold streams and lakes of high altitude and latitude alike are capable of supporting colonies. It may almost be said that in the extreme environments the beaver inhabits, there are in common only water and others of its kind.

Large, webbed hind feet are powerful means of propulsion. The broad, horizontally flattened, nearly hairless tail serves as rudder and balance. The dual coat consists of long guard hairs that mat down into a "diving suit," and soft, shorter inner fur. Flaps behind the strong, orange-colored incisors effectively exclude water during cutting operations under water. Nostrils are furnished with valves, and beavers can retain and conserve oxygen enough to remain submerged for reported periods of five minutes or more. The ears also are equipped with "valves." These all are obvious adaptations for water life. There may be subtler factors of metabolism, body temperature control, ability to receive vibrations under water, and underwater vision, of which there is insufficient knowledge.

There is neither need nor space here for detailed physical description or thorough discussion of life history and habits. On these phases of beaver life, Morgan, Seton, Mills, Bailey, Warren, Grinnell, and others provide most of the known facts. Personal observations and deductions, and acceptable

TABLE 1

Collected Data on Populations, Harvests, Revenues, Trapping Methods, Pelage

State	1946 Population	1946 Harvest	Gross Value	State Proceeds	Method of Trapping	Do Black Pelts Occur?	Do Albino Pelts Occur?
California	25,000	400	$ 4,800	None	Damage permits, licensed trappers	No	No
Colorado	40,000	8,640	$ 272,323	½ private, all public	State trappers	1–1,000	"One since 1941" (1 in 50,000)
Idaho	42,500	7,192	$ 144,147	$45,140	State and farmer trappers	Frequent in No. Idaho	No
Indiana	5,000	None	None	None	None	No	No
Maine	30,000	4,658	$ 130,000	$ 9,316	Licensed trappers	No	No
Michigan	50,000–100,000	9,859	$ 394,360	No figure	Licensed trappers	1–1,000	Not definitely known
Minnesota	40,000–	8,283	$ 172,935	$1 per pelt	Permittees, licensed trappers	Occasionally	Yes, rarely
Montana	No figure	10,200	$ 255,000	No figure	Permittees	1–200	Very rarely
New York	6,358	5,567!	"$ 1 per sq. inch"	None	Licensed trappers	No records—very dark individuals occur	No records
Oregon	30,000	3,005 289	$ 141,940 None	All None	State trappers Indians	1–30	2–3,000
Utah	No figure	3,000 (est.)	Unknown	No data	Permittees	No	No
Washington	100,000	5,248	$ 162,000	All	State trappers	10%	No
Wisconsin	No figure	15,280	$ 702,880	No data	Licensed trappers	1–1,000	None recorded
Wyoming	No figure	10,761	$1,188,925	$ 9,457	State trappers, permittees	5–1,000	No

secondhand information concerning beaver "engineering" and other works, accruing through ten years in Rocky Mountain National Park, Colorado, largely compose the remainder of this article.

Food being of first importance, some observations on feeding are apropos. Generally, summer diet includes much more herbaceous material than that of winter. However, considerable cutting and some consumption of woody plants, even trees of some size, take place in summer. It is known that the rapid growth of incisors in rodents generally requires that a certain amount of gnawing be done to keep a balance of wear and growth. Likewise, in winter, when bark and tender wood are favored staples, rhizomes and fleshy roots of plants growing in frost-free soil or pond muds are eaten in surprising quantity. On many occasions, especially in late fall and early spring, when water is partly open, fresh refuse of herbaceous foods has been found. Once, at midday in early winter, two beavers were under observation for more than an hour as they repeatedly dived, cut pond-lily stems, and brought them out onto the edge of the ice to feed. The upper stems and fragmentary leaves were not used. Incidentally, these animals almost invariably folded the tail under and sat upon it during periods of as much as five minutes on the ice between dives.

Aspen and, in lower elevations, cottonwood are staple food trees in the Rockies. In high-altitude valleys where aspen is scarce or extirpated, willow is used almost exclusively. Everywhere, willow is consumed regularly even if cottonwood or aspen is abundant. In summer it commonly is used as a convenient browse, taken most often from stems growing at the water's edge. Cutting of aspen and cottonwood, involving more time on land, is at a minimum during the summer, perhaps because of greater predator activity.

There are few species of woody plants growing near beaver-inhabited waters which are not used at some time. Cutting of species of alder, birch,

BEAVER ISLAND LODGE WITH WINTER FOOD PILE BESIDE IT

CLEARED PATHWAY FOR TRANSPORTING BRUSH AND POLES FROM AN ASPEN GROVE

THE BEAVER'S TAIL IS FUNCTIONAL

FLAT, AND NEARLY HAIRLESS, THE TAIL IS USED AS A BALANCE AND RUDDER IN SWIMMING, AS A PROP WHILE CUTTING TREES, AND FOR THUMPING THE GROUND OR SPLASHING WATER, PRESUMABLY AS A SIGNAL.

maple, serviceberry, *Cornus, Sorbus,* and *Prunus* has been observed. Cutting or gnawing also has been found on all conifers native to the central Rockies. Use of less-preferred hardwoods and conifers is not necessarily from hardship. Often they are taken while cottonwood, aspen, or willow is still readily available. Such feeding has been described as tonic, parturient, or connected with fertility. Seasonal occurrence of use does not satisfactorily bear out these explanations.

Woody plants are important not only as food, but both directly and as by-products of feeding, in construction of dams and lodges. These structures contain quantities of mud, rocks, vegetable debris, even animal remains, and, in these civilized days, tin cans, beer bottles, wire, saddle stirrups, burlap bags, roofing paper, and other movable items left within reach. The supporting strength and bulk of most structures, however, are provided by poles, predominantly 1–5 inches in diameter and 3–6 feet long.

In initial construction, a dam contains many poles with bark on, cut and transported exclusively, or almost so, for this use. Much fine brush also is used at this stage. Later repairs and gradual increments are likely to be made with poles peeled during the preceding winter.

In late summer or early autumn, the male of the family or his successor ends his summer wandering. The colony starts preparation against the hazards of winter. Dams weakened by summer floods, ignored while constant flow provided stable water depth, now receive attention. New mud is added to the outer walls of the lodge. Winter temperatures change this to the consistency of concrete, providing additional structural strength and making it difficult for land predators to dig into the lodge.

In the vicinity of the home pond, great numbers of trees are cut in the energetic harvest. This is done with little application of skill beyond patient biting and prying of chips with teeth until the tree falls. Allowing for wind, balance of crown and angle of inclination of the trunk are the only acceptable controlling factors in the direction of fall. Many trees entangle with others near by when cut, and are useless unless, by chance cutting, the supporting tree or trees are felled. Several instances have been discovered of trees with crown so tightly enmeshed that when the trunk was completely severed, it dropped from the stump, remaining suspended vertically, responsive to touch or wind, as a free-swinging pendulum.

One instance was encountered where felling of a second tree released one previously entangled. The former was trimmed of branches, the trunk cut into sections and removed. The released tree, still fresh, was left untouched. So much for the fancy that cutting one tree to release another is a studied act. The tree was not a total loss, however. Wapiti made short work of most of the bark and twigs of the neglected tree.

It is common to find cuts of varying depth and height on a tree, as if beavers of different size had worked each at its own level in a race to cut it; or as if a single beaver had sample-cut at various levels until the most comfortable and efficient working position was attained. Trees with half a dozen or more "trial cuts" are not unusual. Winter cutting above deep snow leaves tall stumps, some as high as 6 feet.

Perhaps the beaver's reputed poor vision explains this action. One autumn, I discovered a 5-inch aspen stump about 3 feet high, cut during the preceding winter. Shortly before this observation, a beaver had almost severed it at a lower level before abandoning the job or discovering that this was a stump with only about two feet of trunk as

DEBRIS REMOVED FROM A BEAVER DAM

TREE SPIRALLY WRAPPED WITH WIRE

BEAVERS CHANNELED OUT BARK AND SAPWOOD BETWEEN THE LOOPS OF WIRE SEVERAL FEET ABOVE THE GROUND.

reward for effort that as well could have cut an entire tree.

Cutting on hard deadwood, as suggested earlier, probably is done to condition the teeth, and could be called "exercise cutting." It has been observed where no other apparent purpose could be served, unless there is nutritive or medicinal value in such wood. Pitch-hardened fence posts have been cut at the base, although they were no obstruction or hindrance to work under way. In one case, such a post was cut at three places, below and between three strands of wire, so that three fragments dangled independently, each suspended by a staple. This reflects persistence difficult to explain, [but] that at least the top cut must have been made at [a] time earlier or later than the others, when snow was deep enough for access to that level.

Paul Nesbit, of Estes Park, Colorado, has photographed an unusual example of cutting. To protect a valued tree, the owner wrapped heavy wire spirally around the lower three or four feet. Beavers meticulously cut out a spiral channel between the twists of wire.

In the museum at Longs Peak Inn, near Estes Park, is a small log gnawed into a series of wooden "beads" of croquet ball size, "strung" on a narrow core of uncut heartwood.

Felling is accomplished by beavers singly or cooperatively biting into the trunk until the tree falls. The cut may encircle the trunk, leaving a near-symmetrical stump, or it may be entirely from one side, producing a beveled stump. When the tree is prostrate, branches are cut and consumed or removed for storage or immediate construction work. The trunk is cut into sections, usually up to 4 feet long in trunks averaging 3–5 inches in diameter. Because of natural ground slope toward water, transportation is largely downhill. This makes slightly less astounding some of the pushing and dragging feats of beavers.

The food storage pile consists of log sections, poles, branches, and finer brush, adjacent to the lodge. Larger, tender-barked poles predominate near the bottom. At the top, brushy tips extrude from the surface of the water, where ice grips them to anchor the pile securely through the winter. The beaver has only to descend by a passageway from the living chambers to the floor of the pond, swim to the food pile, cut off a desired portion, and bring it into the den to be eaten. Uneaten remains are released outside beneath the ice. Toward spring, raftlike masses of peeled sticks may be seen through the ice, drifted against the dam.

Materials may be transported, largely by water, more than half a mile upstream or downstream. Overland transportation more than 400 yards has been observed. Cleared pathways often are prepared. Usually they are well-worn dragpaths from groves that recede from the water's edge as cutting progresses. Clearing these paths involves such things as removing sections of fallen logs, cutting trench-shaped troughs across them, or tunneling under them; tunneling through an abandoned dam; removing or gnawing away exposed roots; cutting off projecting snags or brush that overhang or intrude into the pathway; and excavating an inclined ramp down a steep bank to the water.

Water transportation, safer and easier than land

travel, is much used in bringing food and construction material to its destination. If land is level, canals may be dug. The simplest form is a landward extension of a natural indentation in the bank. Earth and stones are excavated with the paws. Roots and buried logs are gnawed away. In extensive flat land, canals may be several hundred feet long. Lateral canals may extend harvesting into various parts of a grove. Dry or watered tunnels often connect near-by canals or laterals. Canals vary in depth from 3 feet or more to a few inches at the shallow end farthest from the pond. They range from about a foot to more than 4 feet in width. Excavated troughs on the pond bottom usually connect the canal with food pile, dam, lodge, and through channelways along the stream axis.

When rising ground or bedrock conditions stop landward extension of a canal, it may be carried farther afield by excavating at a higher level. This involves construction of separate canals connected by portages. More commonly, the canal floor rises gradually, and at a point where water becomes too shallow for safety, a cross-dam is installed, acting as a "lock." Usual objective is a grove of water-loving species of trees, where the high water table provides adequate water for the upper level.

PENDULUM TREE

TIGHTLY ENMESHED CROWN PREVENTS TREE FROM FALLING.

BEAVER CANAL

SEVERAL HUNDRED FEET LONG, WITH A MAXIMUM DEPTH OF ABOUT 30 INCHES AND A MAXIMUM WIDTH OF OVER 5 FEET.

In Windy Gulch, Rocky Mountain National Park, in 1939, there was a dug canal with three successively higher cross-dams, connecting a home pond with a grove of aspens on the east slope of the valley, about 175 feet away. At the grove end, beavers had excavated a natural spring to make a pond about 15 feet across and more than 2 feet deep. A low dam around this pond actually created a fifth level to the waterway, just a few inches higher than the water impounded behind the uppermost cross-dam.

In Mill Creek Valley, a few miles away, other beavers built an arc-shaped dam around a muddy spring that had been an elk wallow. Between this developed reservoir and the main stream below were canals and small pools created by damming the overflow from the higher reservoir. Trees near the upper pool were being cut and transported to the home pond, a hundred yards distant, using this artificial waterway. In view of these observations, stories of beavers diverting stream water into parallel canals supplying high-level ponds downstream, in much the same way man diverts and impounds irrigation water, seem reasonable.

An entire stream course may become in effect

one extensive system of "lock levels." On Cub Creek, Rocky Mountain National Park, in 1938, there were seventy-six impounded levels within about two miles, between its junction with the Thompson River and its source in Cub Lake. The dams ranged from over a thousand feet to only a few feet in length, but none was more than 6 feet high. Some ponds covered more than an acre, others only a few square feet. Almost without exception, water behind one dam backed directly against the dam above.

Dams may be quite long, as on Cub Creek, or may be 10 feet or more in height on the downstream face. Such massive structures usually are the cumulative product of generations of beavers. They seldom are begun in water over 2 feet deep, especially in swift water. Dams may be constructed "from scratch," with all the material brought in, or advantage may be taken of snags, fallen trees, and projecting or island boulders.

The beaver dwelling varies from a simple bank burrow with living chamber above water level to a complex, several-chambered island lodge which may be quite large. One lodge in Cub Creek Valley, at its prime in 1936, was over 35 feet on its longest axis at water level. It stood approximately 8 feet above the surface in water almost 4 feet deep. During a short-wave broadcast from this lodge in 1939, a radio announcer and I, together weighing over 400 pounds, and some forty children averaging about 60 pounds each, put almost 3,000 pounds on it without damage. Incidentally, muskrats were living in tunnels in the outer walls, in apparent mutual tolerance with the beavers. In grass on the lodge just above water line was a spotted sandpiper's nest.

Comment on the complex ecology of the beaver environment can receive only passing notice here. No valid evidence of beaver eating fish is known to me. If such behavior occurs, it is not typical. The beaver probably is as nearly a strict vegetarian as any mammal.

Deeper water impounded by beavers normally provides richer plankton and other fish food. Flanking marshes support increased insect life around beaver ponds. Warmer summer water temperatures and diminished current are detrimental to some species and certain activities of fish. However, fishermen generally consider active beaver colonies an asset on a stream.

Impoundment of water affects man when roads, trails, buildings, wells, and agricultural or grazing lands are flooded. However, the beneficial effects of beaver-made lakes and ponds upon water table and stream flow more than compensate for local damage in ordinary wild lands. Had there not been thousands of generations of beavers to construct myriad dams to slow channel cutting in several of the most picturesque glacial valleys in Rocky Mountain National Park, unchecked erosive forces long since would have reduced them to gullied, impassable, boulder-strewn wastes.

A colorful account of beaver stubbornness in maintaining impounded water was related by the late Abner E. Sprague, of Estes Park. It is repeated here from a manuscript:

In order to build a road to our place in Glacier Basin, we had to cross a brook, of which the pesky beaver had made a slough. To save labor, I used an old dam as part of the roadbed. It was decayed and partly washed out, abandoned by the beaver. I made a small bridge for the water to pass through, and constructed a road, about fifty feet long, over the miry ground. Shortly after it was done, Mr. Beaver found it was a snap to fill under, and at the upper end of the bridge, thus forming quite a pond, but not enough; so to make a good job they began to build the roadbed higher.

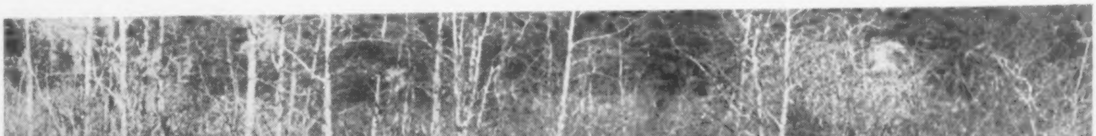
I fought them for a few summers, tearing out their work two or three times a week. Finally, with chicken wire and heavy rock, I kept them from filling under the bridge. Then they built a dam about fifty feet below the road and flooded it again, and again began a dam on the road. . . . I had to tear a hole in the lower dam at least two or three times a week to keep the road dry. I sent men down once who tore out and piled up the entire works; no good—in a week the water was over the road again, and in repair work they had not used a stick of the old dam.

What to do but keep on tearing a hole in the dam I could not think until one day I saw along the road a length of corrugated culvert pipe, left by the National Park road men. I borrowed this iron pipe which was about sixteen inches in diameter, and fourteen feet long. I dug a hole in the dam, and laid the pipe in, making it tight, neither the upper or lower end being supported. Enough water ran through so the material they put in would wash through. I visited the dam for a few times, and passed along the road nearly every day. I began to feel proud of myself in outwitting their chief engineer. But in about a week as I came in sight of the dam, I saw the upper end of the pipe, high and dry, aimed about forty degrees above the horizon and the road covered with water. They must have sent an old fat Beaver or two to sit on the lower end and tip it up, while the workmen stuffed sticks, moss, and mud under the upper end to hold it up. We gave the pipe a bearing the entire length, except about three feet that stuck out into the pond, so they would not build a dam around the end.

. . . . I thought we had them stumped, but not for long. One day in passing, the road was covered with water, and the pipe was jammed full, not a drop of water going through. How did they do it? I found out in about an hour's work cleaning it out. Either by accident or design they had cut a small sprangly lodgepole pine and drawn it into the pipe butt end first; the spring of the limbs holding it in place, they had a foundation. The rest was easy. If the pine was an accident the first time, it was not after that, for that pipe had to be cleared every day, and always one little pine tree was used.

Mr. Sprague's experience truly presents beavers

BEAVER TRACKS IN SNOW, SHOWING COYOTE TRACKS CROSSING AT CENTER



CONICAL MUD HEAPS ON ROCKS IN BEAVER POND, ROCKY MOUNTAIN NATIONAL PARK

as ingenious fanatics about loss of water. An unfortunate incident resulted from this propensity some years ago in Rocky Mountain National Park. A beaver colony flooded private land, requiring removal of the offenders. In urgent cases, the animals are live-trapped for transplanting. On this occasion, a section of dam was removed. A Bailey trap was placed in the opening so a beaver would enter it in placing sticks in the dam. This worked perfectly—except that after one beaver was trapped, others repaired the dam in such a way that the cage was completely submerged. The imprisoned beaver drowned, victim of the well-meaning labors of its co-workers.

Beavers make careful preparation against the necessity of going about in winter, but there is some activity outside at that time. One has but to make frequent winter visits to colonies to find where beavers have emerged from the home pond. This usually is done at or near the spillway, where lowered water level may allow egress, or where thinner ice may be broken through by pushing with head and back. On one occasion in early winter, an almost perfectly round hole some 14 inches across, and about 2 feet upstream from the dam, was found in ice more than 2 inches thick. There were freshly peeled sticks upon the ice beside the opening, where a beaver had sat to feed. How such a perfectly round hole was made is puzzling. The fact that it was maintained through 2-inch ice by regular use reflects the reluctance of beavers to retire for the winter to the confines of the lodge and underwater environs.

Even in deep snow, beavers occasionally flounder about, cutting and eating small trees and shrubs, even while there is a superabundance of food in the home pile, much of which may be abandoned unused in the spring. Tracking has shown that winter travels occasionally cover some distance. Longer trails more often have been observed along pondless stretches of streams, suggesting that wider-ranging travel is more common among lone males which frequently live in banks of deeper holes and are less provident in storing food near the home den. Fred M. Packard in "Beaver Killed by Coyotes" (*J. of Mammal.* **21**, 359, 1940) describes a case of winter predation upon a vagrant beaver, reconstructed from evidences examined by Packard and Gregg.

In the autumn of 1936, some unusual work was observed on Cub Creek. In a roughly circular pond, about 35 feet across, twelve to fifteen boulders rose slightly above the surface. Upon most of these rocks, and at points along the banks, were conical heaps of mud, small sticks, and trash. They averaged 20 inches in base diameter and 12–14 inches in height. One heap, apparently four separate piles fused into one, was nearly 2 feet high, and over 4 feet on the longest base line.

The normal "sign heap" is a flat, "slicked-off" mass of mud almost devoid of sticks and debris, upon which excretion of the castoreum glands is spread, presumably as an olfactory advertisement associated with mating. Usually it is in shallow water or upon the adjacent bank, round to oval in outline, and about a foot in diameter. One on Fall River in Rocky Mountain National Park, about 12 by 20 inches, was unusually large.

The Cub Creek heaps may have been merely excavated material, fortuitously disposed in many similar lots. This seems improbable, in the light of extensive examinations of beaver colonies without discovery of such heaps elsewhere, although innumerable instances of disposal of debris have been observed. The "sign heap" is the most logical explanation of these heaps, justifying suspicion that this was an abnormal manifestation of the reproductive pattern.

Compared with the phenomenal fecundity of some lesser rodents, the reproductive rate of the beaver is low. Its adequacy, however, is attested by cited figures of current populations and harvests.

Young are born in the spring, usually two or three in young females; as many as six in prime maturity. The presence of four developed mammae suggests this as near the number of the average litter, a presumption supported by observations and review of the literature.

The young of the previous year normally remain in the lodge, largely self-sufficient, through the summer of their second year. They live compatibly with the mother and offspring of the current year. The male either leaves or is expelled from this company and spends a presumably pleasant summer as a vagrant, free of domestic responsibilities. This relationship better supports the now generally accepted belief that the species is polygamous than the earlier concept that beavers are strictly monogamous. In any event, it has its points for the male of the kind and, more important, it seems to have succeeded for beavers.

A magnet of wealth that was important in drawing westward the vanguard of a growing nation; a creature wondrous in way of life; at once a benefactor and a nuisance in its stubborn hoarding of water; accepted friend of the fisherman; land-builder and counteragent against erosion; an indomitable race that has defied extinction to become an important economic asset today—the beaver well deserves to be called the magnificent rodent.

A SIGNIFICANT DECADE IN SCIENCE

JOHN W. OLIVER

Dr. Oliver (Ph.D., Wisconsin, 1917), whose special field is the history of applied science and technology in the United States, is head of the Department of History at the University of Pittsburgh. He is chairman of the Advisory Board for the Historical Staff writing the History of Research and Development for the Navy and was formerly director of two Army historical research projects.

SENATOR JOHN C. CALHOUN, of South Carolina, greatest philosopher-statesman of his time, declared late in the year 1841: "We have, Senators, reached a remarkable point in the progress of our civilization, and the mechanical and Chemical arts, which will require a great change in the policy of our civilized nation."

Senator Calhoun was merely echoing what many of his contemporaries were saying during the opening years of that decade. The scientific interests of the young nation were beginning to stir. A whole host of scientists, inventors, and engineers had for years been laying the foundation for a scientific awakening. Not only had they been pleading for a better understanding of science, but, more significant, they were, a century ago this decade, pointing to the great possibilities that lay ahead.

There was abundant reason for this optimism. A brief roll call of some of the nation's top scientists and of a few major inventions of that period may be of interest as we now consider the background of the organization of the AAAS, on September 20 one hundred years ago.

Charles Goodyear had just recently announced his success in vulcanizing rubber—an invention that ranks among the greatest in the history of American technology. Cyrus McCormick's reapers were put on the market in 1840, and he sold them that year for $50 each. Four years later, he had raised the price to $100—and sold fifty-two reapers. Farmers and agricultural inventors were beginning to think and plan in terms of machinery rather than hand tools.

1840

The most significant event of the year 1840 was the founding of the National Institute. Its object, as announced by Joel R. Poinsett, one of its founders, was "to promote science and the Useful arts." Its plan of organization was so comprehensive and so well thought out that it served as a model for the organization of the Smithsonian Institution six years later. In the beginning the idea was to have the National Institute take over the Smithson bequest and administer it for scientific work. Its officers included many prominent government officials—cabinet members, congressmen, and senators. President Van Buren became "The Patron *ex officio*." Joel R. Poinsett, Secretary of War, was its president from 1840 to 1845. Several bills were introduced in Congress aimed at having the National Institute take over the recently acquired Smithson bequest. But Congress would not go along, and, after four years of fruitless effort, the National Institute went out of existence. But it had served a useful purpose: it had awakened an interest in science. John Quincy Adams said he could anticipate no happier close to his public life "than to contribute by my voice and vote in sustaining the National Institute, devoted as it is to the cause of science."

Other important scientific developments during the year 1840 included an announcement by Professor John W. Draper, of New York City University, that he had achieved first real success in taking an excellent photograph of his sister. Also, the first furnace in the United States using the hot-blast method of smelting iron ore with anthracite coal went into operation that year. It was set up in the Lehigh Valley and remained in blast until the flooded Schuylkill River quenched its fires in 1841. In 1841, Douglas Houghton issued his first topographical report of the Lake Superior region, thus informing the outside world of the importance of the copper deposits in that territory.

1843

A major scientific event of the year 1843 was the survey James Pollard Espy—a distinguished mathematician and meteorologist, known as "The Storm King"—made in laying the groundwork for a National Weather Bureau. Winner of the Magellan Prize, awarded by the American Philosophical Society, Espy had been invited to address the British Association for the Advancement of Science in 1840. He was later appointed meteorologist by both the U.S. Army and Navy. He established a series of daily weather observations,

compiled weather maps, traced the progress of storms, and, in 1843, submitted the first Annual Weather Report in this country.

1844

The year 1844 witnessed a number of important scientific developments. This was the year that S. F. B. Morse finally succeeded, after a long series of experiments, and with the aid of Federal money, in setting up his telegraph line from Baltimore to Washington, and ticked out the historic words "What hath God wrought."

The same year, Alvan G. Clark and Sons ventured into the business of manufacturing telescopes. During the year that followed, the company was asked five different times by the Navy Department to grind "a telescope lens more powerful than any in existence." The company soon became known as the most expert lens grinder in the world, and orders came in from all parts of the globe.

The most significant scientific event in 1844 was the holding of America's first scientific congress, called by the National Institute. Several attempts had been made prior to 1844 to organize scientific congresses on the plan of the British Association for the Advancement of Science, but without success. Finally, the National Institute decided to attempt a meeting in Washington on a nation-wide scale. The date was set for the first week in April 1844. Invitations were sent out to members of the American Philosophical Society, the Association of American Geologists and Naturalists, and all other scientific and learned societies. More than one hundred persons were invited to prepare and read papers.

On April 1, 1844, at ten o'clock, the members and guests of the National Institute, led by President John Tyler and accompanied by the National Marine Band, formed in procession and marched to the Presbyterian Church on 4½ Street. After the opening prayer, President Tyler spoke, calling attention to the fact that this was the first general scientific meeting of men from all parts of the United States.

President Tyler was followed by the Honorable Robert J. Walker, United States Senator from Mississippi. In his keynote address, Senator Walker paid grateful tribute to "the untiring votaries of science." "The greatness of our nation," he continued, "depended upon the advances it made in the field of science. Science, by subjecting nature to the wants of mankind had enlarged our knowledge and provided man with increased comforts."

Senator Walker demonstrated a thorough acquaintance with the accomplishments of American men of science. He reviewed in detail the work of Benjamin Franklin in the field of electricity; of Joseph Henry and his current experiments, also in electricity, especially in electromagnetism; of Robert Hare on the calorimeter and the galvanic battery; of Professor John W. Draper; of S. F. B. Morse on the telegraph; of Dr. Alexander D. Bache in his extensive magnetic observations; of Joseph Saxton in providing for a continuous current; of Thomas Godfrey in making the mariner's quadrant of practical use; of Professor James Espy in meteorology; and others. Walker concluded with a stirring appeal to all scientists, urging them to unite their efforts in making "this country the greatest and freest nation of the world."

The meeting lasted from April 1 to April 8, inclusive. There were ten sessions, during which papers on astronomy, geology, natural history, physics, chemistry, meteorology, paleontology, entomology, pharmacology, physiology, and other subjects were presented and discussed. John Quincy Adams, ex-President and at that time a member of Congress, presided over the closing session. He announced that he was proud to have been a member of this First National Congress of scientists, and offered his apology for not having been present at all the sessions, owing to his presence being required at the Capitol.

Too little credit has been given to this early scientific congress, for it marked a distinct milepost in our history of science. Here was a group of men eager to unite the scientific forces of the young nation through the diffusion of scientific knowledge. This effort stands as "a landmark in the scientific and cultural history of the United States."

1845

The year 1845 witnessed a number of developments that might be briefly noted. First, this year marked the beginning of the *Scientific American*, one of the notable scientific periodicals in American historiography.

In February of that year, the Commissioner of Public Buildings in Washington was directed by Congress to negotiate a contract for lighting the Capitol with gas, provided it could be done at an expenditure of not more than one half the cost for lighting the building with oil.

A definite advance in road building occurred this same year, namely, the construction of plank roads. A plank road fourteen miles in length was built from Syracuse, New York, to Oneida Lake. This

new road was hailed as a great improvement over the old dirt and corduroy roads.

This same year, Stephan Fitch introduced a new device for directing several tools successively against a piece of wood (later iron), thereby inventing the turret lathe. This was hailed as the greatest development in machine tools since the slide rest. With the turret lathe began the series of "automatics," in which tool after tool mechanically comes into play. From the turret lathe came the modern automatic-screw machine, and the typically American grinding machine.

For the boot and shoe industry the "rolling machine," invented in 1845, proved of great benefit. It soon took the place of the lapstone and hammer, used in pounding and toughening leather. This, together with the coming of the Howe sewing machine the following year, marked a great advance in leather technology.

1846

This was a momentous year in our nation's history. So many and so important were the happenings that Bernard DeVoto has devoted a whole book of some 540 pages to recording and interpreting them. He entitled it *The Year of Decision* (Boston: Little, Brown, 1943). DeVoto deals mainly with national and geographic expansion, but a similar volume could be written dealing with the scientific, engineering, and technological advances.

Congressman Cathcart, of Indiana, was moved, as several of his colleagues were, to ponder upon the rapid pace with which the nation was advancing. Addressing his fellow-congressmen, in February 1846, he reminded them that

> An omniscient God has given to man control of the elements [referring to the telegraph], the effect of which is to bring the remotest parts of our vast country into practical propinquity. The iron horse, the steam car, with wings of wind, his nostrils distended with flame, salamander like, vomiting fire and smoke, trembling with power, . . . flies from one end of the continent to the other with less time than our ancestry required to visit a neighboring city. . . . Truly it may be said, that with the social influence of these two great inventions [the telegraph and the railroad], all the people of our continent may be moulded into one mind.

The greatest scientific event of this year, however, was the final establishment of the Smithsonian Institution. On August 10, 1846, President Polk signed the bill which, twenty years after James Smithson had written his will, brought into existence one of the greatest scientific organizations in history.

The Board of Regents held its first meeting in September 1846. Within a year, the Regents had organized plans for establishing the Institution. After drafting plans for the building that was to be erected, they elected Professor Joseph Henry, of Princeton, as the Smithsonian's first secretary. No better choice could have been made. For thirty-two years, until his death in 1878, Joseph Henry planned and directed the work of the Smithsonian Institution, firmly establishing it as one of the world's greatest scientific institutions.

This is also the year that marked the appearance of Howe's sewing machine. For years he (and others) had been working on a machine that would combine the eye-pointed needle with the shuttle, for forming a stitch, and the intermittent feed for carrying the material forward as each stitch was formed. The device for feeding the cloth consisted of a thin strip of metal, provided with a row of pins on the edge, upon which the material to be sewed was carried in a vertical position. These, plus a few later improvements, gave Elias Howe a machine that sewed perfectly, at the rate of 250 stitches a minute—seven times faster than by hand. Such a machine was needed to keep pace with the rapid output of cloth that was being produced in this period.

It should be noted that 1846 was also the year when the great Jean Louis Rodolphe Agassiz arrived in the United States. He came to "a land where Nature was rich, but tools and workmen few, and tradition none." With his arrival, American natural history had found its leader. The following year, the Federal government supplied him with a Coast Survey steamer, and he set out upon a series of explorations that was destined to bring honor, not only to himself, but to the nation.

1847

The year 1847 marked a turning point in scientific education. The Sheffield Scientific School, which claims to have been the first to introduce courses in geology, paleontology, and physiological chemistry, and the first to establish an agricultural experimental station, was founded in 1847.

In June 1847, Abbott Lawrence gave $50,000 to Harvard College to be used to encourage the teaching of science, mechanics, chemistry, and engineering. This marked the beginning of the Lawrence Scientific School.

This same year witnessed a revolutionary speed-up in printing. The Hoe Lightning Press set a record by turning out 2,000 small newspapers per hour, an improvement over the nearest rival, the revolutionary cylinder press, which could print only 1,000 papers per hour.

This was also the year that Cyrus McCormick moved to Chicago, built a three-story brick building, equipped it with six forges, employed thirty-three men, and turned out 500 reapers. The following year, he produced 1,500 reapers. Mechanized farm labor had definitely arrived.

1848

The year 1848 was a memorable one in American history. The war with Mexico was drawing to a close; the United States had acquired the second largest amount of territory ever added to the national domain; and the discovery of gold in California set off the greatest migration in the nation's history. The revolutions of 1848 in Europe drove thousands of liberal-minded men out of their native land, and many of them sought shelter in the "Land of Free."

That keen French observer De Tocqueville had recently noted:

If their democratic principle [speaking of America] does not on the one hand induce men to cultivate science for its own sake, on the other it does enormously increase the number of those who do cultivate it. . . . Permanent inequality of conditions leads men to confine themselves to the arrogant and sterile research of abstract truths, while the social conditions and institutions of democracy prepare them to seek the immediate and useful practical results of the sciences. The tendency is natural and inevitable.

In June 1848, the distinguished educator Horace Mann uttered his famous eulogy on the scientific and technologic advance which our people had made, declaring that

caloric energy, gravitation, expansibility, compressibility, electricity, chemical affinities and repulsions, spontaneous velocities—these are the mighty agents which the intellect of man harnesses to the car of improvement. The application of water, and wind, and steam, to the propulsion of machinery, and to the transportation of men and merchandise from place to place, has added ten thousand fold to the actual products of our industry. . . . God, instead of giving us a telescopic and microscopic eye, has given us the power to invent the telescope and the microscope. Instead of 10,000 fingers, He has given us genius to invent the power loom and the printing press. Without a cultivated intellect, man is among the weakest of all the dynamic forces of nature; with a cultural intellect, he commands them all.

Indeed, the time was ripe and the groundwork well laid for such an organization as the American Association for the Advancement of Science.

AT THE MUSEUM

This is the past I crown. Within these halls
The devious currents that have borne me here
Repose. Here are the stars, and here the earth,
Rock, fruit, and beast; and of all lore the first.
Here, in their art and science, lies revealed
The outreach of ten thousand years of men
Struggling, as I, to grasp the nascent truth
Behind the wonder of the world they knew.

These are my fellows, and the urge is strong
To linger here and learn what each has done.
But such were dalliance, with so little known
And so much yet to probe; I must persist
In my own search, nor let their monuments
Beguile me from the work I have to do.

CLARENCE R. WYLIE JR.

THE DOCTRINE OF *AHIMSA* AND CATTLE BREEDING IN INDIA

BURCH H. SCHNEIDER

Professor Schneider (Ph.D., Cornell, 1931), whose special interest is animal nutrition, was a representative of the Harvard Mission in Albania, 1927–29. He was at the Allahabad Agricultural Institute for five years, and is now professor of animal husbandry at West Virginia University.

IN EXAMINING the effect of the doctrine of *ahimsa* on cattle breeding in India, one must first understand its significance. This part of Hindu philosophy prohibits the killing of any animal life. It has far-reaching consequences: it has greatly affected the political policies of Indian nationalists and it is the basis of the nonviolence movement, or *Satyagraha*. When Gandhi was taken to prison in 1942, his last instructions to his followers were: "Do everything possible under *ahimsa*. . ." His assassination may be interpreted as a revolt against this ancient doctrine, which he practiced and advocated. A group of Hindus wanted to fight the Mohammedans, but the Mahatma, who proclaimed *ahimsa*, stood in the way.

The doctrine has its roots in the belief in reincarnation. The orthodox Hindu abhors the idea of killing an animal because he believes it might have the soul of a man from a previous incarnation. The taboo applies most strongly to cattle and to certain other species of animals. The modern Hindu points out that the early lawgivers provided protection for cattle and gave the rule religious significance in order to preserve this essential draft animal. The farmers of India are dependent on their oxen for plowing and all motive power in cultivation. It is reasoned that the ancient wise men thought that in times of drought and famine a flesh-eating people might kill all their cattle for food; then when the rains came again they would have no oxen to cultivate their fields.

Whatever the origin of the doctrine of *ahimsa*, it must be taken into consideration in any plan for general livestock improvement in India. There are few countries in which the lives of the people are so closely associated with their cattle. Approximately one third of the cattle of the world are in India, and, in many sections of that densely populated country, the number of cattle closely approximates the human population. These humped Brahman, or Zebu, cattle have been imported in large numbers into the Western Hemisphere, and they are rapidly increasing in almost all tropical and semitropical climates because of their ability to survive, and even thrive, under difficult environmental conditions to which other breeds succumb.

Western observers have been outspoken in their condemnation of the doctrine of *ahimsa* and its effect on cattle breeding in India. One world authority on livestock breeding told me that he had refused a position in India because he understood that the people would not kill their inferior animals. Thus, he concluded, he could do little professionally, and it would be useless for him to go there. Even among leading Hindus, one can recognize a note of hopelessness with regard to cattle improvement. Although they may be committed to the doctrine of not killing cattle, nevertheless with their background of education in Western practices in animal husbandry, they cannot conceal their conviction that the task of breeding improved cattle in a Hindu society is impossible. I have come across this attitude on many occasions in conversation with employees on government cattle farms, large landholders, and rulers of native states—all interested in cattle improvement, but unconvinced that it is actually possible under India's socioreligious system.

I have been tempted to concur in the outspoken opinion of some other Westerners that the best way to breed improved cattle in India might well be to slaughter all of the poorest animals. Nevertheless, before one has lived many years in close harmony with Hindu cattle breeders, he realizes that it will not be possible in our generation to begin cattle improvement in India by wholesale slaughter, or even by killing a limited number of the worst specimens. Contact with the Occident may bring greater "enlightenment," but one need not hope to see a willingness to kill excess inferior cattle in the near future.

The handicaps of the *ahimsa* philosophy are very evident. The possible advantages have never been pointed out. Breeders everywhere must consider the adaptability of their livestock to the climate, the feeding stuffs available, and the suitability of the animals for the purposes for which they are intended. Also, they must consider the habits, social

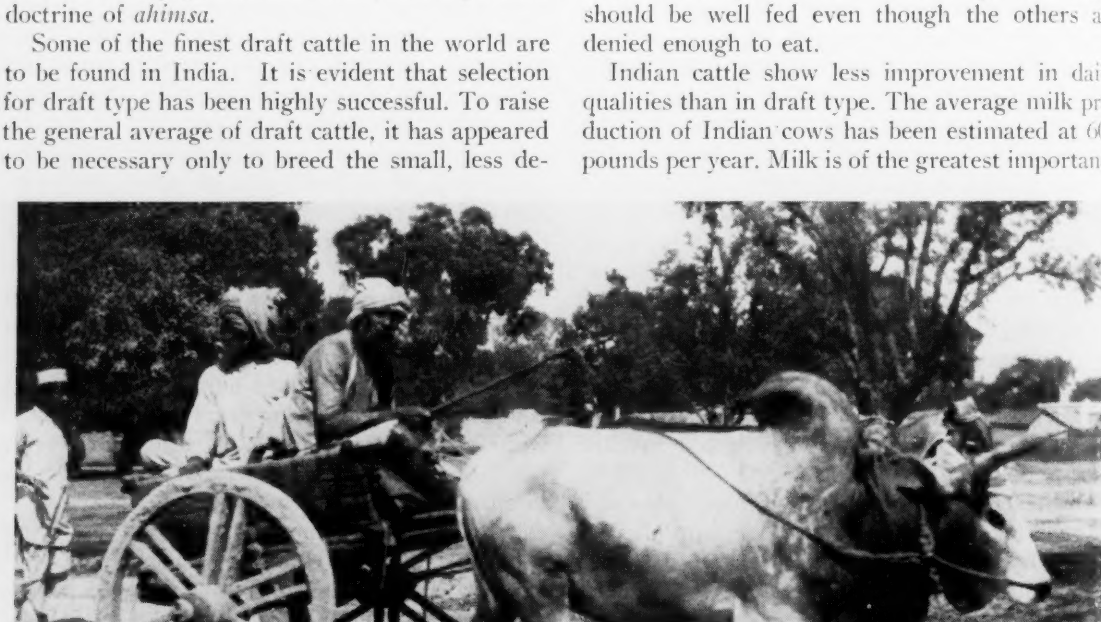
INDIAN ZEBU, OR HUMPED CATTLE

customs, and economic conditions of the people for and by whom the animals are bred.

In India, cattle are bred for draft and for milk production. There has been no need to improve their beef qualities, since a large proportion of the population eat no beef. Hindus, Sikhs, Jains, and Parsis (Zoroastrians), which together compose two thirds of the population of India, all respect the doctrine of *ahimsa*.

Some of the finest draft cattle in the world are to be found in India. It is evident that selection for draft type has been highly successful. To raise the general average of draft cattle, it has appeared to be necessary only to breed the small, less desirable cattle of some sections to the more drafty, improved types found in other parts of the country. The breeding of better cattle in any case must, of course, be accompanied by improved feeding and management practices in those areas that have previously had inferior cattle. Provision for good feeding is one of the imperatives of the livestock industry in India. The improved cattle should be well fed even though the others are denied enough to eat.

Indian cattle show less improvement in dairy qualities than in draft type. The average milk production of Indian cows has been estimated at 600 pounds per year. Milk is of the greatest importance

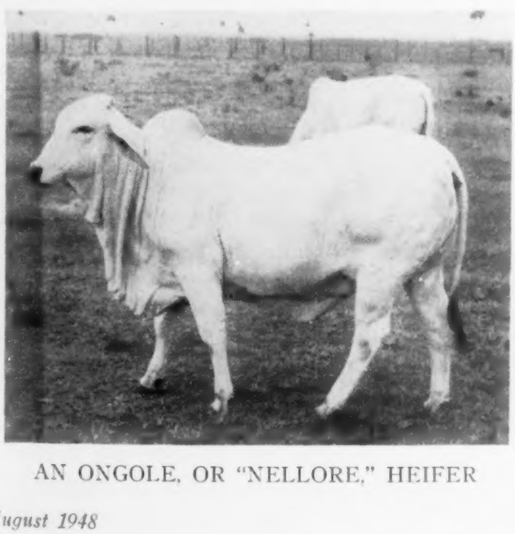


INDIAN OXCART

ZEBU, OR HUMPED OXEN

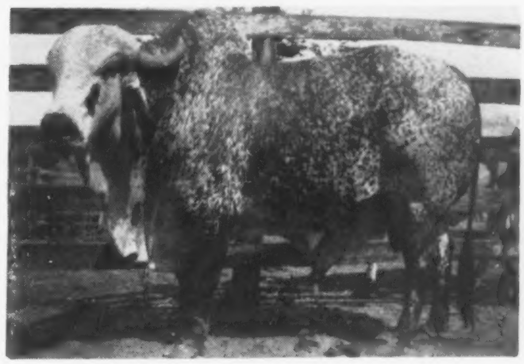
THESE CATTLE ARE THE ONLY PRACTICAL SOURCE OF FARM POWER FOR INDIA'S SMALL FIELDS. HEAVY DRAFT HORSES ARE UNABLE TO WORK IN THE EXTREME TROPICAL HEAT.

because of the doctrine of *ahimsa.* Since meat eating is forbidden, milk becomes the only common food of animal origin that the religion of more than 200 million people permits them to eat. Furthermore, Indian people are very fond of dairy products of all kinds, of which they have several unknown to Americans. It is generally recognized that the proteins of animal origin average higher in biological value, and that such foods contain more minerals, particularly calcium and phosphorus, than most foods of plant origin. There are other dietary advantages of a mixed diet from both plant and animal sources that emphasize further the great importance of breeding for higher milk production wherever milk is the only animal product consumed by great masses of the population.



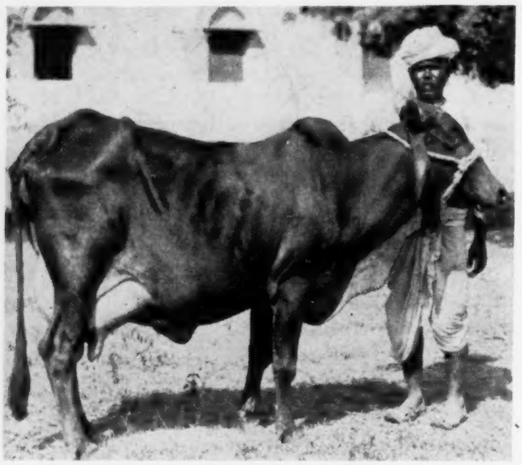
AN ONGOLE, OR "NELLORE," HEIFER

The greatest objection to the doctrine of *ahimsa* is found in the excessive numbers of cattle permitted to live and compete with other animals and with man for the fruits of the land. It is surprising to note the congestion of animal and bird life of all species in some sections of India, which are among the most densely populated parts of the earth. Throughout all seasons of the year, neither man nor beast is able to obtain an adequate food supply. For those engaged in cattle breeding as a business, it is obvious that the excessive numbers of cattle tend to decrease their value, and the attendant shortage of cattle feed increases its price. These two factors make commercial dairying a difficult enterprise unless high-quality milk is sold at high prices in urban areas to Europeans or to well-to-do Indians. The millions of inferior and unproductive cattle that must be fed constitute a huge economic drain on the country.



GIR BULL, ONE OF INDIA'S FINE BREEDS

Under present economic conditions in India few cattle owners have the means to purchase fence or to employ herdsmen to keep their cattle entirely separate from other, inferior cattle. In other countries, also, where public grazing grounds were formerly the rule, general improvement of cattle was not begun until laws were passed requiring individual owners to fence in their livestock. (Many countries having no taboos restricting the slaughter of cattle have fully as poor animals as India.) It is the custom, but not religious law, for all cattle in an Indian village to graze together. Poor cows, good cows, calves of all ages, scrub bulls, and government-approved bulls are all in the same herd. Fencing cattle is not as feasible in India as in some other countries, because each owner often has only two or three animals.

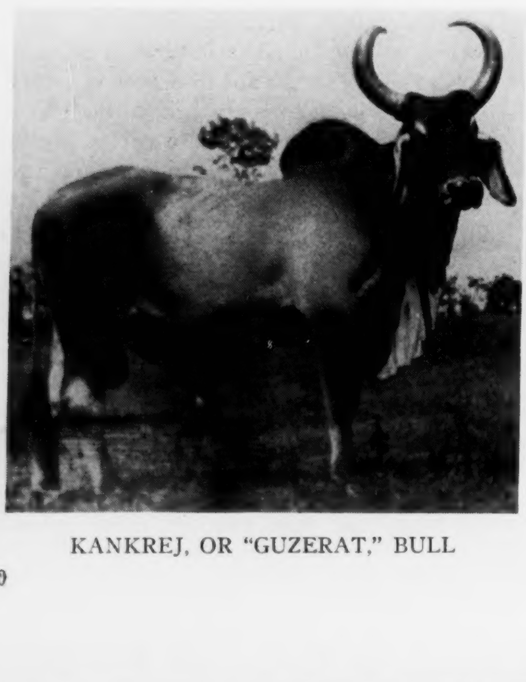


MATAPALUT, WITH "SIND QUEEN"

FOREMAN, ALLAHABAD AGRICULTURAL INSTITUTE. A MEMBER OF THE PANCHAYAT (COUNCIL OF 5 MEN) OF THE AHIR ("CATTLEMEN") CASTE IN THE TRANS-JUMMA REGION.

Fencing materials are expensive, and the relative cost per animal would be exorbitant for the poor peasant.

Any attempt to improve livestock must have the full cooperation of the entire village. Education and confidence are necessary. When public sentiment swings in favor of any reform, pressure may be brought to bear on backward individuals in India as effectively as in any other country. It is not necessarily inherent in the Hindu system that inferior cattle be permitted to propagate themselves promiscuously. Social custom, not religion, has permitted promiscuous breeding. It is possible to remedy this defect without violating the Hindu conscience.



KANKREJ, OR "GUZERAT," BULL

CATTLE improvement may be undertaken from either the lower or the upper end of the scale. In Western countries we have raised the average of our cattle by culling the poorer individuals. We have culled our cattle almost invariably by killing them. This is, of course, very effective in making changes quickly in the average merit of any group of individuals. Consequently, our thinking regarding cattle breeding is such that most of us have the idea that this is the only way to improve cattle. Actually, this kind of mass selection is not very effective in producing better cattle, i.e., in improving animals at the upper end of the scale.

The excellent work in culling at various government farms in India illustrates the effect of culling the poor cows. It has been the custom on such farms to measure progress in breeding for milk production by the average daily milk yield per cow. Improvement of this kind, however, is not

PLOWING CONTEST AT FARMERS' FAIR

a function of breeding, other than that replacements in a herd are obtained from among offspring of cows remaining in the herd after culling. It is possible to increase the average by discarding the poorest producers year by year, yet breed no better cattle. There is evidence that this occurs. For instance, there are records of Indian cattle producing 7,000 pounds of milk per lactation thirty years ago, yet as late as 1932 no better yields had been obtained by breeding. Since that year, changes in feeding and management have resulted in increases in production not due to breeding. Culling out the poorest cows raised the yearly averages of a herd, but did not breed better cattle.

Our Hindu brother, who is pessimistic regarding the improvement of cattle in his country because of a fundamental religious philosophy that he is reluctant to give up, may well ask: "What is accomplished by 'murdering' cows?" For each inferior cow killed, we prevent the birth of an average of less than one inferior calf annually. This practice, therefore, which is opposed to

ahimsa, if carried out wholesale, reduces the numbers of existing inferior animals, but it is not as effective in preventing the propagation of inferior cattle as another practice: castration. Castration can be far more effective in reducing the numbers of poor calves. A scrub bull may sire twenty or more scrub calves per year. Castration is not as offensive to the religious sensibilities of the Hindus as the killing of cattle. It is well known and is practiced in all parts of India. Although there might be difficulty in carrying out wholesale castrations in certain parts of the country, I am of the opinion that there is no part of India where general castration of scrub bulls cannot be introduced, especially if the cooperation of intelligent leading Hindus is obtained. The use of the "bloodless" Burdizzo castrater is particularly helpful in dealing with peasant prejudice. Castration *is* practiced widely. Where it has been objected to on religious grounds, this has been only an excuse advanced by Hindus who did not understand the program to improve their cattle, and who were fearful of the consequences. Without understanding, they might well fear the possibility of being left without adequate numbers of breeding bulls.

Hinduism is not opposed to selection of cattle with the viewpoint of increasing the number of progeny from superior bulls. *Ahimsa* is opposed only to killing; it is not opposed to castration. It is not opposed to selection if it can be practiced without "murdering" cows. Our Western practice of slaughtering cattle culled from breeding herds has made culling, killing, and herd improvement appear inseparable.

KANKREJ COW FROM NEAR AHMEDABAD

It is possible to practice selection by means of castration, and permit superior bulls to sire more

OX TEAM WITH STUDENT DRIVER

THE AMERICAN STEEL PLOW IS GRADUALLY SUPPLANTING THE WOODEN PLOW USED BY 90 PERCENT OF INDIA'S FARMERS.

calves, without coming in conflict with Hindu religious thought. In fact, *ahimsa* permits the more accurate selection of superior bulls than is possible in Western countries. The progeny test has proved itself particularly valuable in recent years with dairy cattle. The increased use of proved bulls is the goal of dairymen throughout the United States. However, it is recognized that many bulls may be rated more favorably than they deserve because their inferior offspring have been slaughtered. This difficulty would never occur in a Hindu community. All heifers strong enough to mature and produce milk are permitted to do so. None would be eliminated without having had an opportunity to obtain a measure of her milk-producing ability. Furthermore, a measure of longevity of production and longevity of life of certain families might be obtained from the records, as no cows are killed even when they are past the age of economical production.

The practice of having public bulls at stud in India would supply the machinery for progeny testing in a way not found in many other parts of the world. It has long been a custom for a Hindu gentleman to honor a deceased relative by giving a bull to the community for public service. It is said that formerly superior animals were given, but it appears that the custom has now degenerated so that only cheap, inferior bulls are purchased and dedicated to the honor of the dead. This function of supplying bulls for public service has largely been taken over by the local governments.

It would be possible to maintain liaison between the peasant breeders and the central breeding farms that supply the bulls. The bulls should be used only in one-bull herds to prevent uncertainty regarding paternity, and they should be moved from time to time to prevent their breeding their own daughters. Bulls may be passed on to other villages or, if they have sired superior offspring in the villages, returned to the central breeding farm as proved sires. All scrub bulls running in the same herds should be castrated. It is also possible that such a well-organized scheme might achieve sufficient prestige in certain areas that service could be refused to inferior cows.

Certainly, tremendous effort and great leadership would be required to launch a comprehensive system of cattle improvement based on progeny testing in the villages surrounding even one cattle-breeding farm from which bulls are supplied. I know of only one such attempt having been made. The plan is entirely feasible, however. Every Indian village has at least one literate man who, with proper supervision, meticulously keeps certain village records. It is far from impossible to conceive of such a man maintaining accurate breeding, production, and other records necessary for progeny testing. Thus, bulls could be regularly proved in all the villages in the area. It is important that such a local area be saturated with good breeding bulls. Such a plan is better than spreading the available bulls out thinly by scattering them over an entire province. It is also desirable that such an area be near the farm on which the bulls are bred. It is possible that progeny testing may not be effective in the beginning if the native cows are very heterogeneous. This is a difficulty that will be progressively overcome, however, generation by generation, as bulls of similar breeding are provided.

Cattle could be improved by breeding the best better. Improving the cattle at the central breeding farm would in turn enable better bulls to be supplied to the villages. Culling and killing of poor individuals would not be a requirement of this plan, for all offspring of every bull would be permitted to live as complete samples of all progeny capable of survival. Proper recording and application of genetic principles can go far toward making the best of a bad situation. Selection would take place on the basis of these samples of germ plasm more complete than anywhere else in the world. Improvement would take place at the top, not by eliminating the lower-producing individuals. Such a system could operate as economically and effectively in India as in any other country, with many points in its favor.

Although the doctrine of *ahimsa* is a handicap to phenotypic selection as practiced in Western countries, where inferior cattle are culled and killed, it appears to be of value to genotypic selection and the breeding of superior animals, provided good records are kept and genetic principles followed. Occidentals consider *ahimsa* a great disadvantage from the Western point of view, but it need not be an unsurmountable obstacle to cattle improvement in India. Newer practices of progeny testing may be carried out more completely in a Hindu society than where there are no inhibitions regarding slaughter of cattle. Genotypic selection for milk production may then be found to be more effective by not practicing close phenotypic culling of both sexes. This should be a challenge for agricultural officers in the newly created Dominion of India.

TECHNOLOGICAL INNOVATIONS AND THE CHANGING SOCIOECONOMIC STRUCTURE

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SOCIETY is in a constant state of change; we are always doing things somewhat differently from our predecessors, living a little differently and thinking a little differently. Although such change has probably been occurring in all societies at all times, prior to the industrial revolution change took place gradually. By contrast, it is particularly noticeable in our modern industrialized society—or so we like to believe—where changes of all sorts are swiftly taking place. Social change has been recognized by every adult who has ever said, "Things were different when I was a youngster," and probably the vast majority of all adults have made some such comment.

It is no accident of nature that social change occurs. Changes in biological life due to mutations perhaps can be attributed to chance factors. But not so with changes in society. Of the factors causing change in our society, one of the most important is technology—important new inventions as well as minor technical innovations, some so minor as to hardly merit patenting.

> Invention is a great disturber and it is fair to say that the greatest general cause of change in our modern civilization is invention; although it is recognized that social forces in turn encourage or discourage inventions. Certainly developments in technology cause a vast number of changes in a great variety of fields. A banker once defined invention as that which makes his securities insecure. Hence a study of the trends of inventions furnishes a broad perspective of many great movements of change and basic general information for any planning body, however general or specific their plans may be (*Technological Trends and National Policy,* edited by W. F. Ogburn).

In this short article it is proposed to trace the effects of technological innovations in only a very limited area—namely, that in the plane of living and the class structure of our society. It will be shown how new inventions and new techniques affect and cause changes in the basic occupational and industrial structure of the nation, and how the plane of living has gradually been raised to the highest average level ever experienced in this world; the possible repercussions of these changes upon the class structure will then be examined. Emphasis is thus placed on the sociological aspects of the picture rather than the economic or productivity aspects per se. (For an excellent discussion of the economic aspects of technological change in the United States, see F. C. Mills: "Technological Gains and Their Uses.")

The data presented here refer to the United States, in which the application of modern technology has been carried to the greatest extreme. The conclusions, however, apply to other industrialized countries, as far as can be determined, and they also give a suggestion as to the future of other less industrialized nations.

TECHNOLOGICAL progress is sometimes thought of in terms of outstanding and newsworthy inventions such as the automobile, airplane, motion picture, and radar. However, the term means much more than simply such few outstanding developments; it encompasses the great multitude of small inventions that improve the efficiency of some minute aspect of a particular job without necessarily introducing anything new. These inventions are seldom newsworthy, and not always patentable. In general, they may be said to consist of two types—procedural and material inventions. Both contribute to increased productivity, defining this term very broadly in the words of W. Duane Evans, "We are interested in determining whether a given job takes more or less labor over a period of time."

In the first category is included much of the know-how of industry and business—the administrative organization, the laying out of efficient and orderly work flows, the establishment of improved labor-management rapport—all of which increase efficiency and productivity. Thus, for example, the introduction of the moving assembly line was originally a procedural invention, and in itself increased the output per worker. Such innovations are not always thought of as inventions, despite the fact that they do ultimately contribute to increased productivity.

The second category—material inventions—includes items and processes that are generally pat-

* This article is not written in the official capacity of the author and is not to be interpreted as representing any official opinions of the Bureau of the Census.

entable, but not necessarily newsworthy. A new lubricating oil, for example, which will permit a machine to operate at increased speed, may cut the labor required at some one particular point in a complex manufacturing process. Or the raising of a new type of seed may permit increased agricultural production per acre without the necessity of increasing the expenditure of man power. Any single one of these inventions in itself may have an effect on the ultimate end product, although by itself it is hardly—if at all—measurable by any of the known measuring instruments. The sum of many such individual items, however, becomes apparent in even the crudest of productivity measures.

The volume and regularity of production can also have an effect on measures of productivity; a large volume of products being produced without interruptions will often require less labor per unit than in the cases of smaller volume or interrupted production. As W. Duane Evans wrote:

> The cement industry is an example. Output per man-hour continued to increase through the year 1942, as production expanded. After the bulk of war construction was completed, cement production dropped sharply, and with it productivity. By 1944, output was only half as great as it had been in 1942, and output per man-hour was 20 percent lower than in 1942.

As can be seen in Figure 1, there have been considerable increases in productivity during recent years. Increases undoubtedly occurred in the years prior to those shown here, but comparable data are unavailable.

Output per man-hour approximately doubled between 1919 and 1939 in all manufacturing industries combined, in mining, in the electric light and power industry, and in railroad transportation. During this same period productivity in agriculture as measured by output per worker increased only about 25 percent, but since it is probable that working hours in agriculture declined, as happened in manufacturing industries, output per hour probably increased somewhat more than 25 percent. In the telephone and telegraph industry (for which data are available only for the period 1935–46) there has also been some increase in productivity.

During the war years productivity increased in some industries and declined in others. Owing to the radical changes in the nature of the manufactured goods produced during wartime, comparisons of the prewar and war periods for manufacturing are not permissible.

As for the future, there can be no doubt but that productivity, regardless of how it is measured, will continue to rise.

Perhaps the single most important hiatus in the field of productivity measures is for those industries which do not produce tangible goods. Manufacturing, mining, electric light and power, and

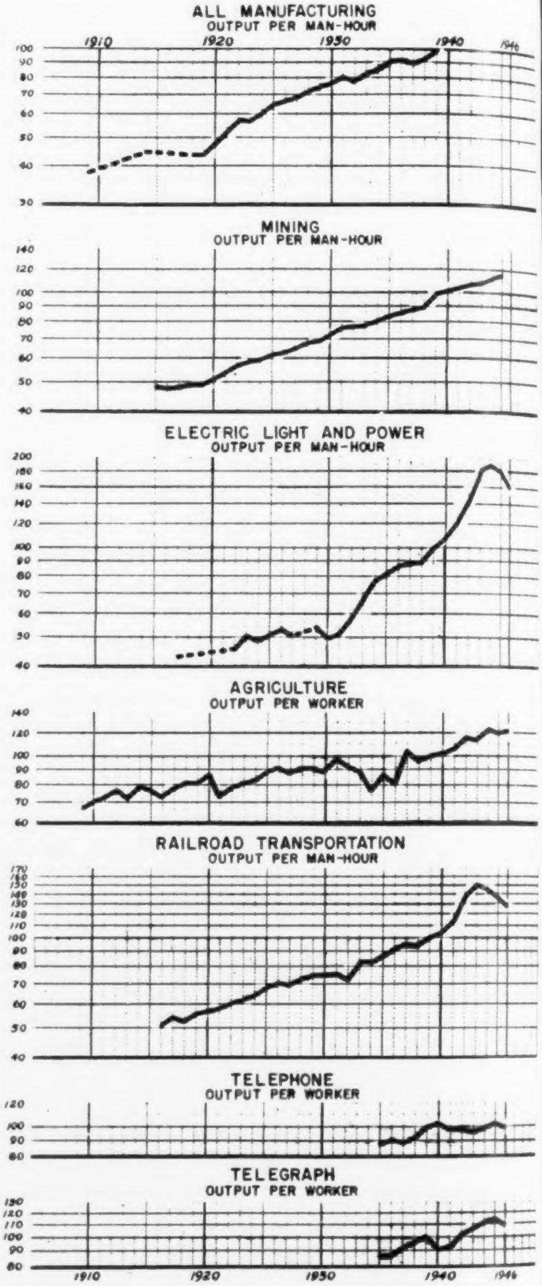

FIG. 1. PRODUCTIVITY INDEXES*

Sources: C. S. Gody and A. D. Searle, "Productivity Changes Since 1939," *Monthly Labor Rev.*, Dec. 1946; *Productivity in Agriculture: 1909–1946*, U. S. Bureau of Labor Statistics, Dec. 1947; *Productivity and Unit Labor Cost in the Telephone and Telegraph Industries: 1935–1946*, U. S. Bureau of Labor Statistics, Nov. 1947.
* (For selected industries, 1909–46. 1939 = 100.)

agriculture all result in tangible goods; railroads and the telephone and telegraph industry are more in the nature of service industries, although they do supply a standardized service which can be measured—ton miles of freight carried, message units provided, etc. For the great bulk of the service industries (the supplying of a service—such as trade, professional or domestic service, etc.—rather than a tangible good), unfortunately there are no productivity indexes, owing in large measure to the lack of a standardized measurable product. Yet, as will be shown, some half of the present labor force is engaged in producing services rather than goods. Thus, in order to appreciate fully the effects of technology and inventions on our society, it will be necessary to devise means of measuring trends in productivity in those industries now engaging half or more of all workers.

The need for improving the present measures, especially those which represent statistical aggregates of several individual products and industries (as that for all manufacturing activities combined) is the second major problem facing students now engaged in this field. The U. S. Bureau of Labor Statistics, which has done most of this work to date, is constantly engaged in attempting to improve its measures.

The thesis of this article is that one of the important elements in precipitating social change is technological innovation. Technological innovations, taken together, have produced the vast increases in productivity, or output per man-hour (or per worker as the case may be). Such radical changes in the efficiency of production and the utilization of man power, in turn, have affected the social structure in various ways. Let us examine several of these derivative effects.

THE CHANGING INDUSTRIAL AND OCCUPATIONAL COMPOSITION

Industrial composition. One of the immediate effects of changes in productivity is upon the industrial composition of the labor force. In Table 1 the industries of the United States have been classified into two major groups: those which produce tangible goods (agriculture, forestry, fishing, extraction of minerals, manufacturing, mechanical industries, and construction), and those which largely produce services (transportation, communication, public utilities, professional service, clerical services, trade and finance, domestic and protective service, government service, etc.). At each date the proportion of the working population engaged in these two broad classes is shown. (Detailed statistics are given in Table 2.)

TABLE 1

Type of Industry	Percentage of Workers					
	1947	1940	1930	1910	1890	1870
Production of goods	44	49	53	62	68	75
Production of services	56	51	47	38	32	25

In 1870, with the relatively inefficient means of production then available, three quarters of all workers in the United States were engaged in industries producing goods, and of these the bulk were in agriculture. (In 1840 almost 80 percent were engaged in agriculture alone.) By 1947, with the remarkable increases in productivity in these industries, less than half—some 44 percent—were engaged in producing goods.

The substitution of more efficient machines and processes for less efficient ones and for man power resulted in the freeing of millions of workers from the necessity of producing the physical goods requested by our society. These workers recruited from farms and factories, as well as the farmers' sons and daughters and other new workers entering the labor market for the first time, plus many of the millions of immigrants, found jobs in the ever-expanding service industries.

Agriculture in particular was able to free workers for nonagricultural pursuits. In 1870 more than half of all workers were engaged in agriculture (including forestry and fishing); in 1947 less than 15 percent were so engaged. In actual numbers there were about as many farmers in 1947 as in 1900, despite the fact that the total population almost doubled in size between these years.

Occupational composition. The occupational composition of the working population permits classifying the workers into two major groups on the basis of the kind of work they actually perform rather than the type of product resulting from their labors. Thus we can delineate two broad groups: those who work mainly with their brains, and those who work mainly with their hands.

In 1947 white-collar workers comprised an estimated 36 percent of all workers (fourteen years of age and over); in 1910 they comprised some 21 percent. The population engaged in manual work (including agriculture) thus decreased significantly from some 79 percent in 1910 to 64 percent in 1947. The increases in productivity during these decades largely affected those occupations in which manual labor predominated and thus permitted increasing numbers to enter commercial, professional, and clerical occupations (Table 3).

TABLE 2

Estimated Industrial Distribution of the Working Population Fourteen Years of Age and Over, United States, 1870–1947*

Industry	1947	1940	1930	1920	1910	1900	1890	1880	1870
Number (in thousands)	60,650	52,022	48,593	41,798	36,634	28,273	22,634	16,887	12,581
Percentage, total	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Agriculture, forestry, fishing	13.0	17.6	21.6	26.5	30.3	36.5	41.7	48.4	52.2
Extraction of minerals ...	1.5	2.1	2.0	2.6	2.6	2.5	2.0	1.8	1.5
Manufacturing, mechanical ind., construction	29.6	29.3	29.0	30.8	29.1	25.5	24.4	22.7	21.0
Transportation, communication	6.7	6.6	7.9	7.4	7.3	6.9	6.2	4.9	4.3
Professional service	6.4	6.8	6.7	5.2	4.7	4.2	3.9	3.3	2.7
All other service and white-collar	42.8†	37.6	32.8	27.5	26.0	24.4	21.8	18.9	18.3

*Alba M. Edwards: *Comparative Occupation Statistics for the United States, 1870 to 1940*, U. S. Bureau of the Census, Washington, 1943. 1947 estimates made on the basis of current (post-1940 U. S. Census) data published by the U. S. Census Bureau and the U. S. Bureau of Labor Statistics.

† Including armed forces and those of unknown industry.

Increases in productivity have affected the service industries and white-collar occupations, as even casual observation will reveal, but probably not to the extent to which the production of goods has been affected. Thus, for example, the introduction of the cafeteria and the self-service grocery store must have increased the productivity of workers in such establishments. The modern typewriter and calculating machine have increased the productivity of some clerical operations. The physician with his automobile very probably is more productive than one equipped with a horse and buggy in that he wastes less time simply riding from one patient to the next. The major problem, then, is to determine just how much increase there actually has been in these various fields in order to hazard some evaluations of the future.

TABLE 3

Socioeconomic Classification of the Gainful Workers, 1910–30, and of the Labor Force, 1940, United States*

Socioeconomic Class	1947	1940	1930	1920	1910
Nonagricultural:					
White-collar					
Professional	6.8	6.5	6.1	5.0	4.4
Proprietors, managers and officials	10.1	7.6	7.5	6.8	6.5
Clerks and kindred workers	18.7	17.2	16.3	13.8	10.2
Subtotal	35.6	31.3	29.9	25.6	21.1
Manual					
Skilled workers and foremen	13.9	11.7	12.9	13.5	11.7
Semiskilled workers	20.6	21.0	16.4	16.1	14.7
Unskilled workers	15.6	18.7	19.8	20.0	21.5
Subtotal	50.1	51.4	49.1	49.6	47.9
Agricultural:					
Farmers, owners and tenants	8.2	10.2	12.4	15.4	16.5
Farm laborers	6.1	7.1	8.6	9.4	14.5
Subtotal	14.3	17.3	21.0	24.8	31.0
Total	100.0	100.0	100.0	100.0	100.0

* Same source as Table 2.

We can predict confidently that tremendous increases in productivity will take place in the future in manufacturing, mining, agriculture, and those other industries for which productivity indexes are already available. And, for some time in the future, the proportion of the workers engaged in producing goods, or in manual occupations, will continue to decline. Thus, increasing numbers (as well as proportion) of workers will be found in the service industries and white-collar occupations.

We may now ask what the probable effects may be in the future, if the service industries should experience vast increases in productivity comparable to those experienced in the manufacturing industries. That there probably will be considerable repercussions upon the occupational and industrial composition of the nation's labor force goes without saying. But what form will these repercussions take? And, furthermore, what might be the eventual effect on other sectors of the social structure?

INCREASE IN THE PLANE OF LIVING

Technological innovations are introduced generally for one of two reasons. On the one hand, the new invention may permit an improved product, either at a decreased cost or at the same cost (or occasionally even at a higher cost). A second reason for introducing a new invention or process is to reduce costs. These are the only two classes of reasons that will induce a concern to go to the trouble of changing its machines or work procedures in order to increase the productivity of its operations. Of course, in certain circumstances a new machine or device may be introduced for health or safety reasons; in other circumstances, a manufacturer may purchase an improved machine simply because such was the only type available when he found it necessary to replace old equipment. But as a rule the prospect of increased profits is the primary motive for introducing new techniques.

The net result of these technological innovations, then, has been a gain in the plane of living of the entire population, as is well known. We are all familiar with the general appearances of such increases in the plane of living, but specific statistics that would measure such changes precisely are largely lacking. It can be pointed out, for example, that the index of manufacturing production (calculated by Solomon Fabricant)—that is, the estimated volume of physical output—rose from 100 in 1899 to 373 in 1939, or almost a quadruple increase. During the same period, the total United States population increased by only some 75 percent—from about 75 millions to 131 millions. Such an increase in the physical output of industry must have been accompanied by an increase in the amount of goods available per person.

Another index of the increase in the plane of living is the estimated per capita value of all goods available to consumers at several periods (based on data by Simon Kuznets). Owing to changes in the purchasing power of a dollar, such value figures have been estimated in terms of 1929 prices. Over the past seven decades or so the estimated per capita value of such consumer goods has about tripled (Table 4).

TABLE 4

Period	Estimated per Capita Value
1929–38	$562
1909–18	444
1889–98	297
1869–78	183

The mechanism by which the accumulated increases in productivity ultimately result in an increase in the plane of living (that is, the volume of physical goods and services available per person) has been described by C. S. Gody and A. D. Searle as follows:

> Output per man-hour, together with average hourly earnings, determines unit labor cost—the wage payments made per unit of output. If average hourly earnings remain unchanged, unit labor cost is reduced as productivity is increased, and prices can be reduced without any decline in profits. Where there is competition, it is likely that savings in labor cost made possible by productivity increases will be reflected in lower prices, which benefit all consumers. If effective competition does not exist, however, and if average hourly earnings are not increased, the result of gains in productivity may be an increase in profits.
>
> Workers benefit through price declines, together with all other consumers. A more direct way in which workers share the benefits of productivity gains is by means of wage increases. As output per man-hour increases, average hourly earnings may also increase without any increase in unit labor cost and without any upward pressure on prices or any reduction in profits. In 1939, unit labor cost in manufacturing industries was 44 per cent lower than in 1919 and wholesale prices of manufactured goods, 38 per cent lower. Average hourly earnings, on the other hand, were 28 per cent higher. The basis for the wage increases and the declines in unit labor cost and prices was the large rise in output per man-hour.

Theoretically, there is nothing mandatory about this process whereby increased productivity must result in an increase in the plane of living. Conceivably, it is possible for increases in productivity to result in increased leisure. Indeed, in this country, simultaneously with the increase in the plane of living, there has also been an increase in the amount of leisure. In 1909 the estimated average number of hours worked per week in manufacturing industries was around 51; this figure fell rather consistently to a low point of 35 hours in 1938. During the war years, of course, the length of the work week increased; but with the resumption of peace the length of the work week has again declined and in 1947 was around 40 hours. At the time of the Civil War the average length of the work week in all nonagricultural industries seems to have been between 60 and 70 hours.

Another indication of the increase in leisure time is afforded by statistics showing the proportions of young and old persons who were workers (gainfully occupied or in the labor force). At the older ages—sixty-five years and over—some three quarters of all the men were gainfully occupied in 1890. In 1947 a little less than half the men in this age group were in the labor force.

Among boys ten to fifteen years old, about one quarter were gainfully occupied in 1890. By 1940 the incidence of boy employment had decreased to such an extent that the U. S. Census Bureau ex-

cluded the age group ten to thirteen years from its 1940 labor force statistics. It would appear that perhaps not over 5 percent of all boys ten to fifteen years of age were employed or seeking work in 1940. The later age of entry into the labor force is, of course, accompanied by a lengthening of schooling and an increase in the educational level.

Thus, the average age for entering the labor force has been increasing over the past several decades, and the average age for leaving has been decreasing. The net result is that the average male who survives beyond age sixty-five today will have spent a smaller number of years in the labor force than he would have several decades ago. In addition, while employed in the labor force he works fewer hours per week.

It would appear, then, that, in theory at least, society can take its choice as to the ultimate goals to be achieved by increased productivity. On the one hand, the working population can continue to work just as many hours per week and just as many weeks out of the year and just as many years out of a lifetime as it always has and enjoy a continuously increasing plane of living. On the other hand, a society can elect to maintain its plane of living at any given level and enjoy its increased productivity in terms of increased leisure. Of course, any combination of these two alternatives is also possible. Whether the nation is now in the position of having to make a choice can be debated. The fact does remain, however, that the various labor groups in the United States have already exhibited an interest in increased leisure.

According to E. M. Bunn, about three out of four manufacturing establishments had formal paid vacation plans in 1945–46 for their plant workers who had had at least a year's service. Almost nine out of ten establishments provided paid vacations for office workers with a minimum of a year's service. In contrast, paid vacations in 1937 were provided for plant workers by only one in four manufacturing establishments, and for office workers by eight out of ten establishments.

It must be remembered that there is some positive relationship between productivity and volume of production (varying from industry to industry); accordingly, it is possible that certain measures designed to increase productivity are not worth introducing unless there is an increase in volume of production. Theoretically, then, in a case of this nature, perhaps the benefits of increased productivity must be taken in the form of an increase in the plane of living rather than in an increase in leisure. No adequate study has been made of this area.

TECHNOLOGY AND THE CHANGING CLASS COMPOSITION

An income distribution of the total population at any given moment of time can locate an individual at some particular point—such as, for example, in the upper 10 percent, or the lower 25 percent. As we know, however, "class" involves not only the person's position in the economic scale but also his attitudes toward his position therein and the attitudes of others toward him. Therefore, unless information is available to permit determining the attitudes and to state precisely at what points a continuum of incomes can be cut meaningfully, it is impossible to state statistically what the class composition of a nation is.

No data are available that would permit exact description of the class composition of the United States either at present or at any time in the past. Three general types of statistical data are often used as indexes of class composition. These are occupational data, income and plane-of-living data, and educational data. There is some positive correlation among all these indexes. Each has considerable limitations upon its use and interpretations; nevertheless, the use of any one or a combination of these items does permit some analysis and generalization with regard to certain aspects of the class composition.

Effects on the class composition. We have seen how technological innovations and increasing productivity have radically altered the industrial and occupational distribution of the nation, how they have increased the plane of living and the average income level, and how they have led to considerable increases in educational levels. We have also seen that these factors are the ones often used for differentiating the population into classes. We can infer, then, that the class composition has probably also changed gradually.

However, most of these available statistics have grave defects especially apparent when attempting to study historical trends; accordingly, it is next to impossible to state exactly how the class composition of the nation may have changed. It was seen, for example, that the estimated per capita value of goods available to consumers (in 1929 dollars) about quadrupled in the period between 1869–78 and 1929–38. But we have no notion of whether the distribution of such values at both dates was approximately similar, or whether some segments of the population experienced no changes, whereas other segments more than quadrupled their per capita receipts.

Implications of class composition. Despite the fact that we cannot establish precisely the size of

the population in each class either today or in the past, we do know that there are considerable differences among the classes with respect to many very important facets of the social structure. And, speaking very generally, we have reason to believe these differences assume varying degrees of importance as the social composition of the nation varies. Thus we know, for example, that a century ago and more, when the great majority of the population was dependent on agriculture for its livelihood, agrarian problems were among the most important facing the nation; problems of the industrial element were of importance only in localized areas.

As industry and commerce became more important, the tariff question emerged as one element of contention between this group and the agriculture segment. And, finally, as manufacturing and industrial development grew to its present size, industrial problems assumed ever-increasing importance. Whether a new set of problems centering about the growing importance of the white-collar group will become of predominant importance in the future is a question well worth considering.

Class differences. Our knowledge of the differences between the classes with respect to many social phenomena is derived from two main sources. Certain class differences have been observed on the basis of data obtained from the United States decennial censuses and certain other types of large-scale surveys that have covered the entire country or a major segment of it. Other differences have been observed by studying small local communities and selected groups of individuals. Each investigator has had to organize his study along the lines permitted by the data and interpret his results in terms of the types of populations he was able to study. As a result, very few of these studies are sufficiently comparable in design so that their results are additive, and so as to permit exact calculation of the size of the class

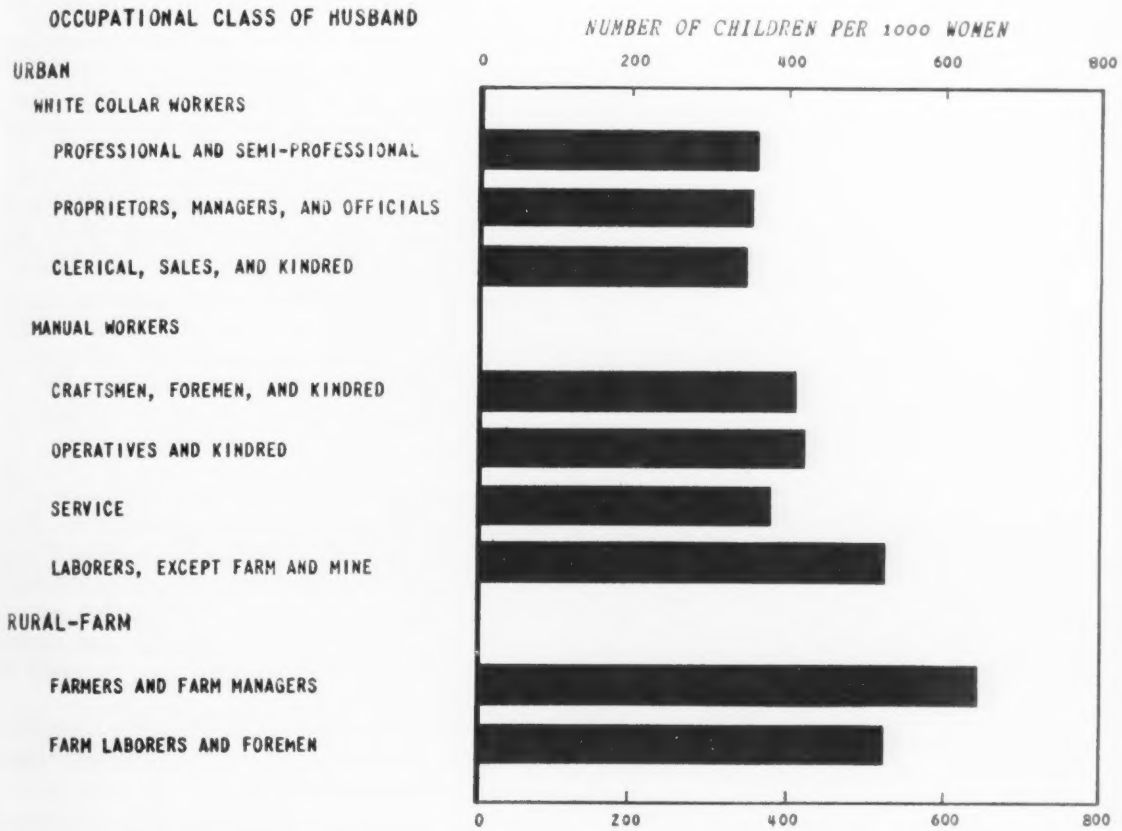

FIG. 2. BIRTH RATE BY OCCUPATIONAL CLASS OF HUSBAND, U. S., 1940

Standardized number of children under five years of age per 1,000 native white women, married once, husband present in household. Standardized on basis of age distribution of total native white women, married once, husband present. Data from *Differential Fertility, 1940 and 1910, Women by Number of Children Under 5 Years Old*, U. S. Bureau of the Census, Washington, 1945, Table 41.

differences observed. Nevertheless, certain differences have been repeatedly observed—differences that are almost always in the same direction. Because of this we can speak with confidence about the nature of some of the class differences even though their present and past sizes are undetermined and we cannot analyze the trends in these differences.

Demographic differences. These are the differences that have been observed in the way people act with regard to the population phenomena described below. The way people act indicates their underlying attitudes and beliefs; hence, although we cannot always understand these attitudes by simply observing the actions, we are sure that differences in action do indicate differences in underlying attitudes.

Differences in the birth rate have existed for a very long time. Figure 2 shows differences in fertility among women classified according to occupation of husband in the United States in 1940. Some studies have classified the population according to rental value of home, and others have used different indices. The same general pattern of inverse relationship between fertility and class level has been observed in practically all such investigations, some of which studied the population of over a century ago.

Inverse relationships have also been observed consistently between the class level (however measured) and the death rate, as the data in Table 5, from Dublin and Lotka, illustrate.

TABLE 5

Social-economic Class	Standardized Death Rate per 1,000 Males (1930)
All gainfully occupied males	8.7
Professional men	7.0
Proprietors, managers, officials	7.4
Clerks and kindred workers	7.4
Agricultural workers	6.2
Skilled workers and foremen	8.1
Semiskilled workers	9.9
Unskilled workers	13.1

The same inverse relationship is found with the infant mortality rate. Among a sample of urban families studied by R. M. Woodbury, in which the father's income was under $450 per year, the death rate of infants (deaths under one year of age per 1,000 live births) was 167; families in which the father's income was $1,250 a year or more experienced a death rate of 59.

The marriage rate is also inversely related to social class in so far as degree of formal education can be considered as an index of class. The higher the educational level, the greater is the percentage of native white women past the child-bearing age (forty-five to forty-nine) who have remained single, as data from Irene B. Taeuber and Hope T. Eldridge show (Table 6).

TABLE 6

Educational Level	Percentage Single
Grade school: 1 to 4 years	4.1
7 and 8 years	7.6
High school: 4 years	12.5
College: 4 years or more	30.1

Attitudes. It is impossible to describe fully the various attitudes of the different classes of a population. About the only data in existence which claim to be valid cross sections of the total population are those obtained from the various opinion and attitude polls. Whether these materials can be analyzed so as to afford a real knowledge of class differences and attitudes is difficult to say, since relatively little work has been done in this direction. General perusal of some of the data released by various organizations suggest the following two very general conclusions.

When persons are asked a question of immediate and direct economic interest to themselves, they tend to reply in a manner favoring themselves. Consider the example from the American Institute of Public Opinion (January 12, 1947).

Question: Many unions of factory workers are going to ask for a pay increase within the next month or two. Do you think these factory workers should get more pay?

Occupation	Percentage answering yes
Farmers	19
Professional and business	31
White-collar	37
Manual workers	50
Union members	59

Although there is room for argument as to how accurate the above figures are or exactly how they should be interpreted, there can be little if any doubt but that the underlying attitudes of the manual workers—the working class—differ significantly from those of the white-collar and farmer groups, and that this question was answered in terms of self-interest.

Another question that may illustrate the role of assumed self-interest is from the Fortune Magazine Poll (November 1946).

Question: The Political Action Committee of the C.I.O. (usually called the P.A.C.) has been active all over the country in supporting candidates for political office that labor approves of and opposing those labor disapproves of. On the whole is the P.A.C. the kind of organization you would like to see continued, or not?

	Salaried executives	Professional men	Wage earners
Answers			
Continued	16%	23%	31%
Not continued	72	67	37
Don't know	12	11	32

The above question also illustrates the second very general conclusion that seems permissible—that among the poorer classes the proportion of the population who have no opinion seems to be considerably greater than among the richer or more highly educated segments of the population. Whether these members of the poorer classes truly hold no opinions, or do not care to impart them to the interviewer, is a subject which requires much further study.

Consider the proportion of "no opinion" answers to the following question in which the respondents have been classified by educational level (American Institute of Public Opinion, January 3, 1947):

Question: Do you think Russia will cooperate with us in international affairs?

Answers	No opinion
College education	7%
High school	11
Grade or no school	23

In so far as a knowledge of the pertinent facts in any given issue helps the person to form an opinion or attitude, it would be expected that the "upper" classes would contain smaller proportions of "no opinion," since formal education is rather highly related to class level. And, in regard to many issues, the better-educated and more prosperous elements have a greater factual knowledge than do the others. A very illuminating study of this aspect of class differences is that of Genevieve Knupfer, "Portrait of the Underdog."

There appear to be class differences in child-rearing habits also, although definitive analyses have not as yet been made; the extent of membership in clubs and other voluntary associations appears to be positively related to class level; sex attitudes and behavior vary among the different educational levels, as shown by Kinsey and his associates; indeed, there probably are significant class differences with respect to practically every aspect of human behavior.

As we have seen, inventions and increasing productivity ultimately affect the class composition of a society, and the attitudes held by the various classes. Not only may the attitudes of the society as a whole be affected by changes in the proportional weighting of the different classes, but the attitudes themselves may be affected directly by the technological changes, or indirectly through the increase in leisure afforded by these innovations. Thus, for example, the growth of Parent-Teacher Associations and the increase in the employment of women are both in part due to the various inventions that have decreased the amount of housework necessary and so afforded women free time to devote to other than housekeeping activities. Unfortunately, our knowledge of the ultimate effects of technology upon attitudes is still very limited. Theoretical formulations exist, but only very limited research has actually been carried out.

Before closing we must note that the relationship between technology and attitudes is reciprocal. To a considerable extent the adoption of new devices and the full utilization of existing ones are inhibited by both irrational and so-called rational attitudes, as has been shown by Bernhard J. Stern. Perhaps the best current example of this is the housing problem. Our society wants more housing, but prevailing attitudes do not permit the production of standardized prefabricated houses on a mass-production basis. Society has accepted such mass production in the automobile and in the newer durable consumers' goods (refrigerators, radios, etc.), but not in housing.

IMPLICATIONS

Perhaps the single most important reason for studying technological change is to afford society a mechanism for predicting the social changes which are expected to occur, and to formulate such policies as circumstances may seem to warrant. For change in itself is disconcerting, notwithstanding the fact that the American people pride themselves on their so-called desire for the "newest" and "latest." Change is often considered "bad" for no other reason than that it is change. Thus the oldsters of every generation when speaking of "modern youth" are sure that the youth of today are not up to the standard of youth when they were youngsters, for the "youth of today" are different. Hence, any thinking that will permit a society to better adapt itself to the inevitable changes which will occur—changes stemming in large measure from technological innovations—will be better able to meet such changes.

Specific discussion and "planning" are called for with respect to the areas previously described. By "planning" is meant only achieving full cognizance of the ultimate developments in a given area, and evaluating them in the light of society's

values. Thus, it is possible for the "planner" to predict that such and such will occur, and that various alternative action programs are possible, depending on the ultimate goals or values of society. If and when society adopts some ultimate goal, then that program can be implemented which will most closely fit the impending changes to such goal. This is not to say, however, that goals and values will necessarily be decided on a conscious and rational basis, but rather that every society definitely does have values, and that these values often change with the passage of time.

DAYLIGHT SAVING

Now man has urged the heavens to his need,
Spurred forth the sun, harnessed the restive moon,
Coralling time like an unbridled steed,
Displacing midnight, and transposing noon.
The stars must wait to mount the Milky Way,
Slacking their silver pace, and as they climb
Man tries to trick them; to expand his day
He meddles with the secret springs of time.

Reverse the dawn and dusk! Recall the sun
At midnight; toy with time; it shall not matter.
Implacable, the hours glide, one by one,
Beyond man's paltry strength to grasp or shatter.
Time will elude him still. Not in his power
To cheat death by a single, stolen hour!

MAE WINKLER GOODMAN

THE HUMAN BRAIN IN THE LIGHT OF ITS PHYLOGENETIC DEVELOPMENT

FRANZ WEIDENREICH

Dr. Weidenreich, a native of The Palatinate, Germany, has taught anatomy at the Universities of Strassburg, Heidelberg, Frankfurt am Main, Peiping, and Chicago. Since 1941 he has been associated with the American Museum of Natural History.

THE discovery of the remains of Peking man in the cave of Choukoutien, and evidences of a relatively advanced culture at the same site, confronted the paleoanthropologist with a new, unexpected, and vital problem. The find of ash layers and burnt stones and bones revealed that the man who lived there had knowledge of fire; and the find of stone implements, some of them skillfully chipped, proved that this man was already an able artisan.

On the other hand, the anatomical record of the skulls shows that the cave dwellers represented a very primitive type, morphologically inferior to any fossil human type unearthed up to that time. The cranial capacity of the first skull to be found is not much over 900 cc. Davidson Black, who described the first finds, had no scruples about identifying the human individuals whose bones were dug out together with the cultural objects as the bearers of the Choukoutien culture. However, Marcellin Boule, the French paleontologist, thought otherwise. He argued that a human individual whose brain was not larger than a little over 900 cc could not be credited with the degree of intelligence that would be necessary to produce such a highly developed culture. From this premise, Boule deduced that a man with the physical appearance of modern man must have lived contemporaneously with Peking man, and that this advanced human type, not Peking man, must have been the bearer of the culture of Choukoutien. Boule regarded Peking man with his small brain as a savage brute who was hunted and killed like any other game by an unknown, more advanced human type. The irony of the implication that the mental and cultural superiority of this man would be evidenced by his chasing, killing, and probably also eating his physically and culturally less advanced fellow-man, did not embarrass Boule. In all the years during which the cave of Choukoutien has been explored, no trace of a second human type has ever come to light; therefore, there is no reason to doubt the identity of Peking man and the culture of Choukoutien. But Boule's objection brings up a general biological problem that has broader implications than may at first be seen.

Is it possible to infer from the size of the brain the degree of intelligence and cultural efficiency of its bearer, regardless of whether this bearer lived several hundred thousand years ago or lives today? Almost everyone, layman as well as scientist, seems to be convinced that such a correlation is a well-established fact. Some time ago I came across a pamphlet, published in 1934, which was written by an English physician. In the author's opinion the only factors that determined man's evolution since his beginnings as a primitive primate are environment and natural selection. But his starting point is the premise "Cranial capacity is a fairly accurate measure of the mental status from the most primitive primates to *Homo sapiens*." The self-confidence with which this statement is made is typical.

Of course, there is no doubt that the size of the primate skulls from the lemurs of the Eocene up to modern man has greatly increased. Compared with the great apes, the average cranial capacity of modern man is three times greater—*ca.* 1,350 cc against *ca.* 450 cc. Skull II of Java man (*Pithecanthropus erectus*), the smallest skull of an adult fossil hominid thus far found, has a capacity of only 775 cc (Fig. 1, *B*). This is 43 percent smaller than the average skull of mankind today, or a little more than half the size of the brain of an average American male adult. The cranial capacity of that Java man corresponds to that of a child of today of eleven to twelve months of age. The average capacity of the skull of Peking man is a little over 1,000 cc. This is still one quarter less than the average capacity of modern man. The apelike ancestors of man are unknown. The fossil Australopithecinae of South Africa, which are regarded by Broom as forms lying directly in the human line, have a cranial capacity of not more than *ca.* 450 cc. This is about the average capacity of living anthropoids. If Broom is right in his phylogenetic ranging of the Australopithecinae, then the human brain would have increased considerably since the

Fig. 1. Casts of the interior of the skull cavity, showing size and form of the brain and its surface pattern from the left side. *A*, adult gorilla; *B*, *Pithecanthropus erectus* II; *C*, Neanderthal man from La Chapelle-aux-Saints; *D*, modern man. About 1/4 natural size. The brain of Neanderthal man had already attained the size of that of modern man, but the contour of the latter is more circular, and it has developed a distinct hump at the vertex.

evolution of man from the South African ape men.

However, we do not know of any fact which proves that the mere increase of the size of the brain is tantamount to an advance in mental ability. Is modern man really more intelligent than Peking or Java man or any great ape only because his brain is larger? This is what Boule's argument implies. More than sixty years ago Marsh claimed that the mass of the brain, particularly that of the hemispheres, has increased considerably in some mammalian orders since early Tertiary times (Fig. 2). Tilly Edinger, who quite recently studied the evolution of the brain of the horse from Eohippus, its earliest recognizable ancestor in the Eocene, to the modern equine form, found that its brain, especially the neopallium, has been enlarged, although the horse did not change its basic organization. This suggests the question: What about the progress of the "mental" qualities of the horse? Is there any indication that the enlargement of the brain implies a greater advantage in the struggle for survival and that the horse only survived because its higher intelligence bound to the enlargement of the neopallium, conducted it safely for millions of years through the perils lurking about it? Since the horse has survived, it must have been equipped from the beginning and at all times with a certain quantity of brain sufficing to let it find its food and evade its enemies. Were the living conditions of earlier horses much easier? Did they therefore require less attention and precaution than in later times so that the horse could survive in earlier times with a minimum of neopallium, whereas later on it could do so only because the brain enlarged in the meantime and gave it a greater chance? The extinction of the horse in North America was already an accomplished fact by the late Pleistocene, although the animal had already acquired a well-developed neopallium.

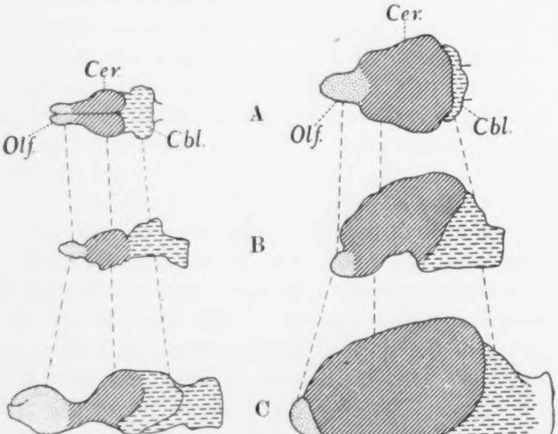

Fig. 2. Enlargement of the brain in mammals from Eocene up to today. *Right*, living animals: *A*, dog; *B*, pig; *C*, rhinoceros; *left*, Eocene type of corresponding mammalian category: flesh eater, small ungulate, large ungulate. All reduced at same rate. *Olf*, olfactory bulb; *Cer*, neopallium (hemisphere); *Cbl*, cerebellum and medulla oblongata (cerebral end of spinal cord). (After Henry Fairfield Osborn.)

On the other hand, increase of body size is always accompanied by an increase of brain size. The elephant, for example, has a brain that weighs almost 5,000 gr, and the brain of one of the big whales (Fig. 3) weighs around 10,000 gr. But the increase of the body alone cannot be made responsible for the growth of the brain; for, in proportion to the weight of the body, the whale has a much smaller brain than man. The whale has 1 gr of brain substance for 8,500 gr of body substance, whereas man has 1 gr of brain substance for 44 gr of body substance. Nevertheless, man is far surpassed in this respect by the dwarf monkeys of South America, the marmosets, which have 1 gr of brain per 27 gr of body substance; and man is surpassed even more in these proportions by the capuchin monkey (Fig. 4) with 1 gr of brain substance for 17.5 gr of body substance.

Therefore, neither the absolute nor the relative size of the brain can be used to measure the degree

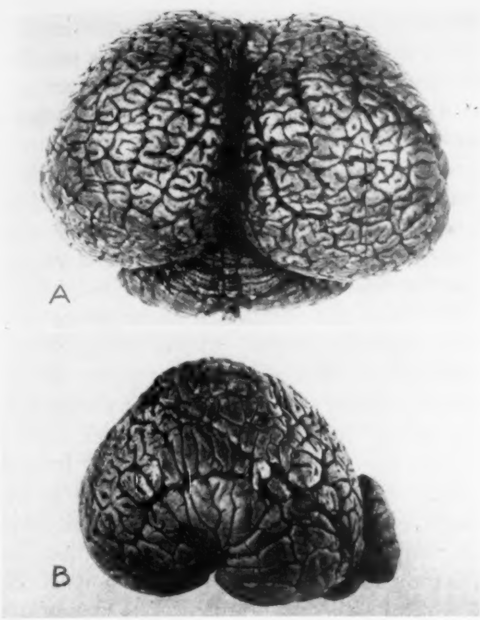

Fig. 3. Brain of a whale viewed from above (*A*); from the left side (*B*). The entire surface of the two hemispheres is partitioned by innumerable large and small fissures, which do not allow any identification with those in *Cebus* (Fig. 4) or chimpanzee (Fig. 5). Note also special form of the hemispheres—they are almost globular, although broader than long and very high. 1/4 natural size. (After F. Tilney.)

of mental ability in animal or in man. So far as man is concerned, the weights of the brains or the volumes of the cranial cavities of a hundred celebrities of all branches of knowledge all over the world have been listed (Spitzka and others). At the bottom of those lists are Gall, the famous phrenologist, Anatole France, the French novelist, and Gambetta, the French statesman, each with about 1,100 cc brain mass. The lists are topped by Dean Jonathan Swift, the English writer, Lord Byron, the English poet, and Turgeniev, the Russian novelist, all with about 2,000 cc. The latter group has nearly double the amount of brain sub-

Fig. 4. Brain (two hemispheres) of a capuchin monkey (*Cebus*) viewed from above. The surface is not corrugated except for a few fissures on both sides. About 1/2 natural size. (After G. Retzius.)

stance of the first group, although the size of their respective bodies does not justify such great differences. Now our mental test! Had Turgeniev really twice the mental ability of Anatole France? If not, and if Turgeniev's body was not of elephantine proportions, which structure of the brain was increased in Turgeniev and the others to such an extent that it answers for the surplus weight, obviously dispensable even for uncommon psychical functions? Gall, Anatole France, and Gambetta, together with innumerable modern human individuals of all races, could perform mental deeds by means of a brain mass which does not surpass the brain mass of Peking man. In addition, the enlargement of the brain does not seem to have reached its climax with modern man. Provided the paucity of the material available warrants such far-reaching conclusions, the climax may already have been reached in the Neanderthal stage, the evolutionary stage that precedes that of modern man (Fig. 1, *C*). In any case, the average cranial capacity of the Neanderthal skulls available is greater than the average capacity of modern human skulls. On the other hand, it can be stated that, at least for the majority of modern mankind, there is no increase of brain mass when compared with that of Neanderthal man.

Thus far only the enlargement of the brain, as such, has been dealt with. There is in addition, however, another kind of enlargement of the brain that goes hand in hand with it but obviously depends—at least in part—upon the space of the cranial cavity in which the brain has to be accommodated. This enlargement concerns the surface of the hemispheres. Their outer layer, the cortex, is the most vital part of the brain, since it is the main seat of the nervous cells. Together with their connecting fiber system, it represents the switchboard at which all the stimuli arrive from the periphery of the body and from which reactions and impulses emanate. The expansion of the cortex is brought about by a series of foldings which transform the originally smooth surface into a confusing maze of wrinkles (convolutions and fissures). The greater the number of wrinkles, the larger will be the surface area and the more cortex elements—cells and fibers—could be accommodated. Among the primates, the more primitively organized the forms, the smoother the surface, and the more advanced the forms, the more complicated are the convolutions; the climax is reached in man.

Fossil brains, of course, have not been preserved, but the wrinkles of the surfaces of their hemispheres have left their imprints on the cerebral

side of the brain case, where they now appear as ridges. These negatives tell the same story as the wrinkles of the brain surface of living creatures (Fig. 1). In primitive fossil forms, the pattern of the wrinkle system appears simpler than in more advanced primitive forms. This can only be ascertained provided the wrinkles have left their marks on the walls, which, however, does not occur in all cases. In any event, the hominid pattern differs from the simian pattern. Each of the living anthropoids has its characteristics (Fig. 5), although they have some features in common that make possible their distinction from man (Fig. 6).

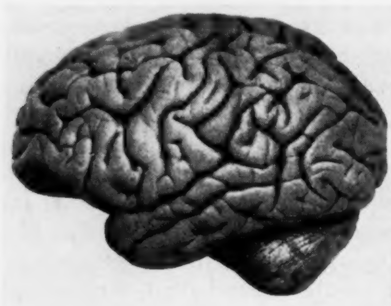

Fig. 5. Brain of a chimpanzee viewed from the left lateral side. The surface of the hemisphere is wrinkled throughout. About 1/2 natural size. (After G. Retzius.)

Again we have to state that when discussing the pattern of the surface of the hemispheres, primates and man do not differ from other mammalian orders with regard to presence and abundance of the wrinkle system. In the evolution of the horse, the surface of the hemispheres is smooth in the beginning, and their folding becomes more complex the more the neopallium increases in size. Indeed, this process is an almost general phenomenon, and any explanation confronts us with the same difficulties encountered in the attempt to interpret the increase of the brain size itself. We are lost again if we suppose that the number or the complexity of the wrinkles is correlated with progress or perfection of the mental faculties. The capuchin monkey (Fig. 4), which many experimental psychologists and physiologists regard as equal in docility to any highly gifted chimpanzee (Fig. 5), possesses an almost smooth brain surface, whereas the chimpanzee has a wrinkled one that comes close to that of man. The whale and its relatives, however, again steal the show. They have the greatest number and finest wrinkles all over the hemispheres, and the most intricate arrangement, in the whole animal kingdom (Fig. 3). Many human and comparative anatomists have spent years classifying and identifying all the individual convolutions and fissures occurring in the different orders of mammals. It may be possible to distinguish the brains of gorilla, orangutan, chimpanzee (Fig. 5), and man (Fig. 6) from one another by this method, but only main fissures which represent the first folds both in ontogenetic and phylogenetic evolution are recognizable and comparable. Their location obviously depends on the manner and rate of growth of the hemispheres in relation to that of the brain case. Except for this basic pattern, the variability is enormous; it differs greatly with regard to the number, length, breadth, depth, and arrangement of the wrinkles, not only between two individuals of the same zoological group or race, but also between right and left hemispheres of one and the same individual (*cf.* the two brains of modern man in Fig. 6). It is therefore hopeless to expect any result or even indication with regard to certain mental faculties—degrees or differences—by such a comparison.

In the face of all these facts it is hard to understand why people cannot get rid of the idea that mere size or configuration of a special convolution or fissure must give a clue to the mental qualities in general and to those of certain individuals in particular. The desire to gaze into the crystal ball seems to exist not only among the clients of fortunetellers but also among scientists.

One hundred and fifty years ago phrenologists indulged in palpating the head and the skull to look for bulges on its outside; from their presence or absence they deduced the presence or absence of psychical qualities which they regarded as

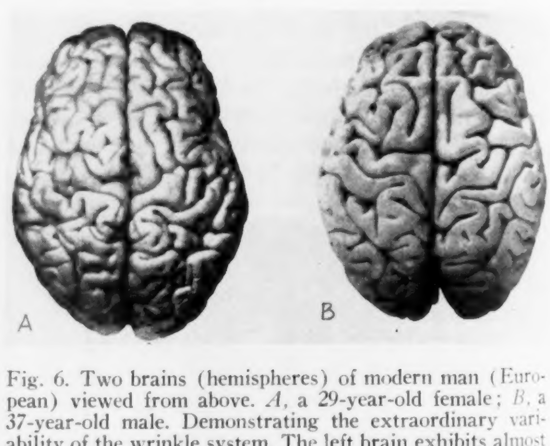

Fig. 6. Two brains (hemispheres) of modern man (European) viewed from above. *A*, a 29-year-old female; *B*, a 37-year-old male. Demonstrating the extraordinary variability of the wrinkle system. The left brain exhibits almost double the number of convolutions and fissures of the right brain, and its convolutions are correspondingly narrower. Note, in addition, the differences in the extension and arrangement of the wrinkles between the left and right side of the same brain. About 1/4 natural size. (After G. Retzius.)

strictly localized. The basic conception of the phrenologists which underlay this reasoning has not changed in the course of time in spite of the great progress made in our knowledge of the structure of the central nervous system. The search has only shifted to a different type of indicator of hidden ability; now it is supposed to be manifested by bulges on the surface of the hemispheres themselves, which allegedly tell of right- or left-handedness, the faculty of speech, musical or mathematical talents. One wants to find out whether certain human races differ from others in their wrinkle patterns and whether those differences are indicative of spiritual inferiority or superiority, etc. Of course, there may be more justification today for such a search than in former times. Owing to pathological incidents and experiments, it is known that certain areas of the cortex are the centers of certain perceptions and impulses. They can be localized fairly accurately, and their nature can be defined. It is furthermore known that those centers are marked by characteristic microscopic structures which permit identification wherever they may be found. It is also known that those differentiations in the structure are not at all bound to the convolutions as such but only to certain areas of the hemispheres, regardless of whether this area is smooth or wrinkled and, if wrinkled, to what degree or extent. As a matter of fact, surgeons are able to remove large portions of the hemispheres, which may result in a temporary or permanent loss of sensibility or power of movement if certain areas are affected. But the defect, as such, does not necessarily interfere with psychical functions or endanger purely mental ones. Modern "psychosurgeons" deliberately destroy fiber systems of the frontal lobe, widely considered the main seat of the intelligence, effecting a return to almost normal reasoning power of some persons formerly considered incurably mentally deranged.

Therefore, the claims of paleoanthropologists, for instance, to the effect that Neanderthal or Peking man was right- or left-handed, was able to speak or write or could only stammer, all deduced from shallower and narrower or deeper and broader impressions on the inside of the brain case, have no scientific basis even if the interpretation of the imprints could be accepted as correct.

If the variation of a normal human brain from average to almost double its usual size and the multiplying of the individual wrinkles of the hemispheres have no bearing on mental functions, how can a strictly localized bulge produce such an effect? On the other hand, thorough studies have revealed a surprisingly perfect equality in weight and surface area between the two hemispheres of the same individual, irrespective of all eventual inequalities in details of their forms (Le Boucq, H. H. Donaldson). Unilateral bulges are the result of asymmetries of the brain case or, if they are bilateral, the result of changes of the whole skull form. Artificially deformed heads or skulls enclose brains with abnormal bulges, depressions, or asymmetries on the surface of one or both hemispheres, according to the character of the applied deformation. But it has never been reported that the people so deformed behave conspicuously differently from others of the same population with normal skulls; nor do they show any change in their mental faculties.

The faculty of the brain to adjust its form and that of its main parts to any form enforced on the enclosing brain case by altering body conditions is extraordinary. One of the most characteristic features of all known earlier hominids, including Neanderthal man, is the flatness of the brain and brain case in proportion to the length, irrespective of the size of the brain and the capacity of the brain case (Fig. 1). In all modern human races—whatever the brain size or skull capacity—the vertex region is distinctly elevated (Fig. 1, *D*) and the length of the skull base is shortened. These differences are the effect of a sharp bend of the base of the brain case (Fig. 7) which runs in a straight line in quadrupedal animals (Fig. 7, *A*), even in the great apes (Fig. 7, *B*). Early hominids (Solo man) are in an intermediate position in this regard. The transformation of the modern human skull is the final result of a change in the static and dynamic conditions entailed by a more perfect adaptation to man's upright position (Fig. 7, *C*). That this alone is responsible for the alteration of the form of brain and brain case can be deduced from the fact that in whales and related forms (Fig. 7, *D*)—regardless of the size of their brains—almost exactly the same change takes place, with one remarkable difference—that is, the bend is reversed. The base of the brain case is curved upward in accordance with a perfect adaptation of the skeleton of these animals to fast and powerful swimming and diving.

All recorded facts indicate that neither the size nor the form of the brain or the surface of the hemispheres or their wrinkle pattern in general or in detail furnishes a reliable clue to the amount and degree of general or special mental qualities. Nevertheless, there seems to be a parallel between the expansion of the hemispheres, the advance of the bodily evolution of man, and the increase of mental qualities. But no one can tell what the nature

of those correlations may be. In any case, this is a problem that principally concerns neurologists and psychologists. Fossil human material can, if at all, furnish only circumstantial evidence. There have been suggestions that blood supply may play a decisive role in the differences between the psychical functions of notable brains and ordinary ones. If this is so, it may also be true for the difference between the brains of early hominids and those of modern man. Indeed, the holes and gaps of the bony case which lodges the brain and through which both arteries and veins pass are distinctly wider in modern man than in early hominids. There is a definite tendency for these ways of communication between the cranial cavity and the outside to widen in the course of evolution as part of a general trend to reduce the thickness of the cranial bones, which are surprisingly massive in early hominids. In general, the passages through the brain case are distinctly narrower in skulls of

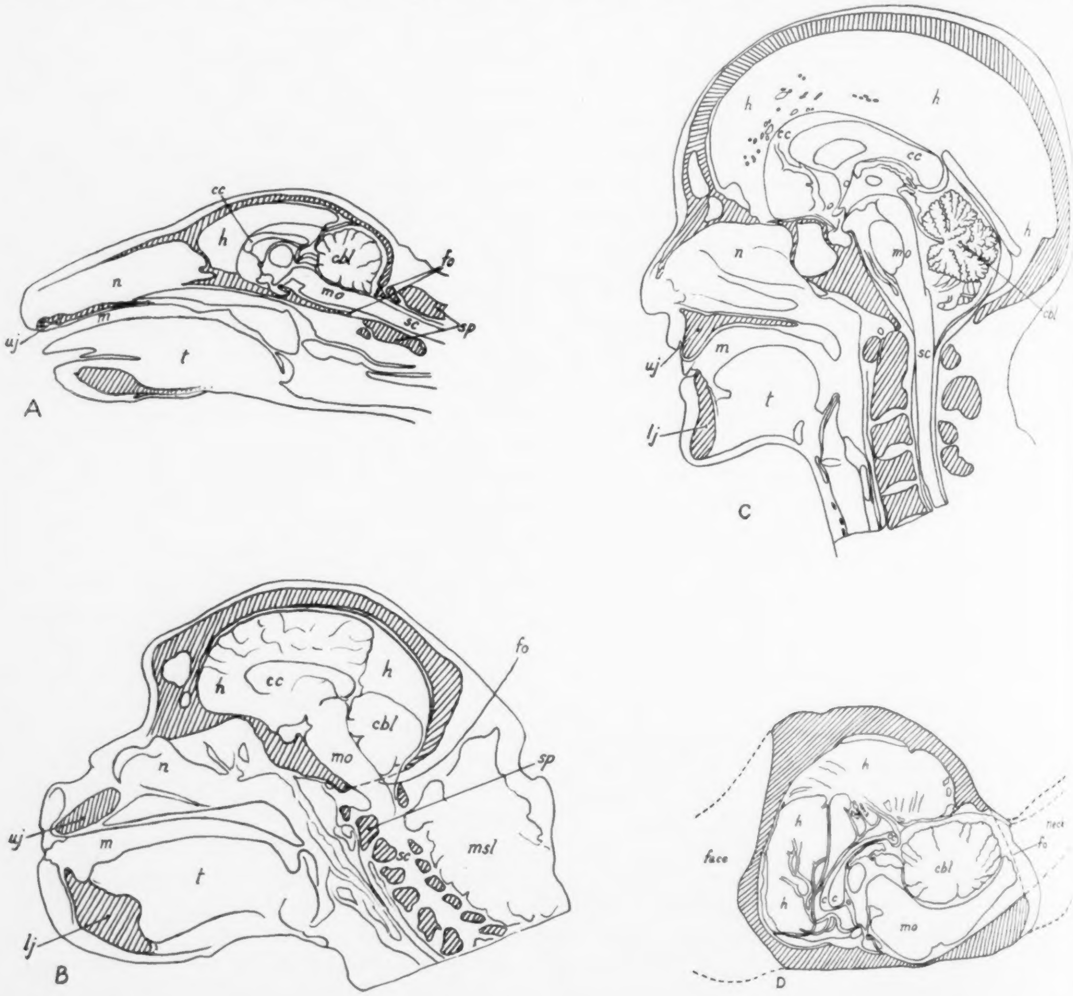

Fig. 7. Demonstrating on mid-sagittal sections through the head the extent to which form and orientation of the brain and its parts depend upon the form of the enclosing brain case and its adaptation to a change in the posture of the body. *A*, example of quadrupedal horizontal orientation of brain case and spine, adult lemur; *B*, example of semierect posture, young chimpanzee. *C*, example of perfectly erect posture, adult European. *D*, example of a fast-swimming and diving animal, adult dolphin. In *A*, spine and spinal cord meet brain case and brain directly from behind. In *B* they meet brain case and brain from behind and below. In *C*, they meet brain case and brain directly from below. In *D*, they meet brain case and brain from behind and above. The base of the brain case (*hatched as the entire brain case*) forms a straight horizontal line in *A*; a straight inclining line in *B*; a downward bend to nearly a right angle in *C*; and an upward-directed curve in *D*. The brain and its parts: (*cc*, corpus callosum; *h*, hemispheres; *mo*, medulla oblongata; *sc*, spinal cord) closely reflect in course and topographical relations the changing conditions. (*A*, *C*, and *D* after F. Hochstätter.) *A* reduced to 1/2 natural size, *B* to 1/4 natural size, and *C* and *D* to 1/3 natural size. Other abbreviations; *cbl*, cerebellium; *fo*, occipital foramen; *lj*, lower jaw; *m*, mouth; *msl*, cervical muscle; *n*, nose; *t*, tongue; *uj*, upper jaw.

races morphologically more primitive than others. But no data with regard to differences in their width are available for small- and large-brained individuals.

Another point may be of some interest. It is known that in lower vertebrates the mass of the spinal cord is greater in proportion to that of the brain than in mammals. Even compared with the great apes (gorillas), the mass of the spinal cord of modern man seems to be much smaller. This suggests that the brain tends to "swallow" the spinal cord and thereby to bring more reflex centers under control of consciousness. This certainly broadens the basis of psychic reaction.

As to the size of the brain, its white substance (medullated nerve fibers) profits most by the enlargement. But the great accumulation of myeline in these fibers used as insulation material may not be its only purpose. Hollow spaces within the bones are used in all mammals as storage rooms for fat and for the lodging of organs of blood formation. It may be that the central nervous system not only has a nervous function but also serves as storage for some stuff which is an essential in the metabolism of the organism.

To return to our starting point: the course of man's bodily evolution can be clarified by the study of fossil human remains if it is done by people trained in comparative human anatomy. But studies made on skeletons alone will never enable us to make statements about either the mentality of the individuals concerned or about mental change or progress over a period of time. Cultural objects are the only guide so far as spiritual life is concerned. They may be fallacious guides, too, but we are completely lost if those objects are missing. And the closer we come to more primitive stages, the less likely we are to discover cultural objects.

THE SCIENTIST

As from the darkened and bewildered deep,
Soundless and slow the ocean currents rise,
And breast the night, and climb into the skies,
Till black waves break against the whitened, steep,
Desired cold continent of the moon, and leap
(The starry lights about them, all the dyes
Of heaven flashing where the spent spray flies)
And, foaming earthward, fall at length to sleep—

So man above the ocean of his days
By search and knowledge climbs until he sees
About his head the stars. About his knees
The world awaits, and when he's home again,
Though he grow humble, earthly all his ways,
A shining's there, and the high stars remain.

HAROLD LEWIS COOK

THE BASIC POSTULATES OF PSYCHOLOGY

W. EDGAR VINACKE

After receiving his Ph.D. from Columbia in 1942, Professor Vinacke spent two years with the Civil Aeronautics Administration in research on the problems of selection and training of aviators. He has been assistant professor of psychology at the University of Hawaii for the past two years.

THE science of psychology has been developing and growing for almost seventy years. Its origin, in modern terms, is generally dated from the year 1879, when Wilhelm Wundt established the first psychological laboratory at Leipzig. That historic event marked the culmination of a long gestation, during which psychology was grown out of physics, physiology, philosophy, and even astronomy. Wundt started psychology off on its extensive experimental career, but the psychology of today would hardly be recognizable to him and his colleagues. A great deal of the period since 1879 has been devoted to controversy, as everyone knows, between a succession of comparatively narrow viewpoints. The most famous of these in America are existentialism, functionalism, behaviorism, and gestalt psychology. Psychoanalysis and related schools should be added to this list, although they were developed by psychopathologists, since they have profoundly influenced (and frequently annoyed) psychologists of all kinds.

It is not my intention to review these controversies. That has already been well done by half a dozen psychologists. Such writers usually end by pointing out that each school makes its valuable contribution, but that most psychologists are eclectics and do not subscribe to any single viewpoint. They suggest that the future will evaluate various theories and ultimately combine them into a unified system. Perhaps the time has come to begin this task. This article is an attempt to formulate the main outlines of psychological thinking as it exists today, not with respect to specific problems, but in terms of the axioms, so to speak, that have become established during its formative period. Students in psychology classes frequently worry over the apparent lack of agreement among psychologists, and other scientists have reactions varying from amusement to disgust in this regard. My thesis is that, actually, all modern psychologists stand on approximately the same general platform, which may be called the "psychological viewpoint."

It is to be hoped that the formulation of this viewpoint will serve a double end. On the one hand, it may indicate that there *is* a "psychological viewpoint," the fruit of past thinking about human behavior. Thus, other scientists may come to understand better what psychologists are thinking nowadays. On the other hand, it may serve the function of an examination of where we stand on historical issues.

Many omissions will probably be evident. Some, to be sure, are intentional, either because no general agreement as yet exists or, perhaps, because psychology today does not deal with such matters. Running the risk, then, of incompleteness, the following postulates are advanced to define the modern "psychological viewpoint."

I SHALL develop thirteen general propositions which seem to me to characterize modern thinking in psychology.

Postulate 1: *The material of behavior.* The same fundamental variables are present in the total behavior of all human beings, although all degrees of these variables may be observed; differences between individuals are largely differences of degree rather than of kind.

The foregoing postulate is not intended to convey the idea that all people are alike; far from it. Apart from the fundamental variables, of course, there are innumerable other variables that may be unique to an individual, a group, a sex, or a race. The argument, on this point, largely concerns the formulation of what constitutes a basic variable. The problem is to isolate and state these variables in cooperation with the other sciences of man.

For the present purpose, a fundamental variable may be regarded as a broad, generalized determinant of behavior in the organic structure of man, or in the world that confronts him, or in a relationship between organism and world. A fundamental variable is, so to speak, a starting point in behavior.

The main point here is that psychologists think in these terms. Glance at any of the leading textbooks in introductory psychology. One finds treatments of a wide range of subjects, from emotion

and motivation, through sensory processes and perception, thinking, learning and memory, etc., to individual differences and personality. In all these the assumption is tacitly made that, although people differ, they all function in the same general ways and face essentially the same problems.

The following examples illustrate this basic feature of psychological thinking:

a) *Organic drives.* Although no final list of organic motivating factors has been presented, and although different terms are used for the same phenomena, we can say that the sex, hunger, thirst, and oxygen-inspiration drives are universal, leading to positive approaching behavior. Contrasted with these are others leading to negative activity, such as excretion and avoidance of injury. The ultimate role in individual behavior of such drives is another matter; at least they enter into the development of each individual to some extent. They are starting points on the organic side of long chains of behavior processes.

b) *Group adjustment and institutionalized behavior.* All individuals are confronted by other people and organized group codes, if only during the helpless period of infancy. All individuals are influenced to some extent by these factors. The fact that established codes or norms confront the organism is a starting point on the side of the external, real world.

c) *Learning, memory, and higher thought processes.* All individuals are modified by dealing with the environment, retain some, at least, of this experience, and make use of it in dealing with later problems. Apparently, some specific laws can be stated in this connection, such as that forgetting is most rapid immediately after learning, and then slower; and that two tasks done in succession and similar to each other are less efficiently learned than two dissimilar tasks done in succession.

d) *Sensory processes.* All people possess variations of the same general sensory functions within quite specifiable limits. Thus, the range of human hearing is 20–20,000 cycles per second, and the average reaction time to a visual stimulus is 200 ms, which is somewhat slower than reaction time to sound. (In this discussion, we have not considered some special qualifications, as with regard to feeble-minded or other exceptional persons who would represent the extremes. *Cf.* Postulate 3.) Examples *c* and *d* represent factors of organism-world relationships as determinants of behavior.

Postulate 2: *Uniqueness.* Every individual, as a total personality, is different from every other individual, as a result of past experience in conjunction with heredity, because no combination of factors is ever precisely the same for any two individuals.

The foregoing axiom is not a contradiction of the first, but a further, and essential, development of it. The chief argument is the dividing line between an individual and general characteristic, or how general a psychological law can be. As G. W. Allport has pointed out, "A general law may be a law that tells how uniqueness comes about." The latter kind of law is necessary, as the modern and complex study of personality is repeatedly demonstrating.

We can say that psychologists do not ignore the uniqueness of the individual, even when thinking in terms of Postulate 1. They recognize that the interplay of basic behavior variables, cultural and group influences and disciplines, and "accidental" situations and experiences add up to unique total personalities, if only because the combinations and permutations are so enormous in number. This feature of the psychological viewpoint is a prime reason for the inclusion of long chapters on personality in modern textbooks, which are otherwise framed in terms of Postulate 1. The more advanced student is far more extensively indoctrinated in terms of Postulate 2. The modern development of the field of personality is largely concerned with it.

Postulate 3: *Normality.* "Normal" behavior is a relative matter; it is that behavior which characterizes most people within a given cultural group (with certain privileged exceptions, such as the artist). "Abnormal" behavior is extreme deviation from a given average point in terms of a defined variable, or group of variables.

This postulate is primarily statistical. It explodes the fallacy that people are separable into distinct types (a feature of naïve, everyday thinking, by the way) and states that people who differ widely, say, in introversion-extroversion, are simply extremes of a continuum where most people are neither markedly "introverted" nor markedly "extroverted." In other words, the normal probability distribution is now regarded as characteristic of most, if not quite all, psychological variables.

In discussing "normal" people, therefore, the psychologist is making no qualitative distinctions, but is simply dealing with the average person. Since it happens that modern psychology is primarily a development of Western culture, we should qualify Postulate 3 in terms of its frame of reference. Implicitly, the American psychologist is thinking about that behavior characteristic of most American individuals (in terms of Postulate 2—i.e., over and beyond the fundamental variables). It would be a valuable project for a group of Hindu or Chinese psychologists to develop a normal psychology of Hindu or Chinese people. To some extent this kind of development is no doubt occurring in Soviet Russia. In any event, it is evident that normality, as we have defined it, may be very different in different cultures.

The concept of "abnormality" is a corollary of that of "normality." It is that behavior which deviates to an extreme degree from the average,

whether with respect to intelligence, adjustment to the group, emotional-motivational behavior, etc. The "abnormal" is therefore only an arbitrary and fairly extreme distinction from the normal.

Postulate 4: *Physiological concomitants.* Every manifestation of behavior in an organism (here, the human being) occurs in association with some physiological change; no human behavior can take place without some change in the organism.

Not much comment is required here. Stemming largely from the arguments of the behaviorists, modern psychological thinking rests upon the assumption (as yet it is often only an inference) that the organism functions in terms of its structure and the properties of its organized tissues. Even a thought, however original or untraceable in origin it may seem, nevertheless depends upon some activity of the nervous tissues. Even an emotional impulse is somehow a chemicophysical response.

Postulate 5: *Heredity-environment.* The human organism, like all living things, is the joint product of heredity and environment; no aspect of the behavior of an individual is entirely independent of either heredity or environment.

One of the great controversies in psychology, and one vital to most psychologists, concerns the contribution to behavior of "nature" and "nurture." In a sense, there are two aspects of the problem. On the one hand, the effects of hereditary and environmental factors on behavior may be rather neutrally examined; on the other, an inevitable assessment may be made of their relative importance. In any event, despite wide divergences of opinion on the latter point, nearly all psychologists today recognize the joint contribution to development of both factors. If the current standard textbooks be regarded as representative of psychological thinking, Postulate 5 must be regarded as valid.

Evidence for the truth of Postulate 5 is singularly difficult to obtain, to be sure, because of the difficulty of controlling and measuring the development of human beings. One of the wisest and most careful of psychological observers, R. S. Woodworth, has made an admirable study of the evidence. Reviewing the studies made of twins (identicals and fraternals), foster children, and children in institutional homes, he presents a clear analysis of the present status of the problem. The total picture is one of the inseparability of hereditary and environmental factors. In any event, we cannot now say that either heredity or environment is *the* important factor in development. Both jointly determine ultimate behavior, in a complex interaction, in a multiplicative (since neither can be zero) rather than additive sense. It must rest here until further work has been done.

Postulate 6: *Socialization.* Human behavior of any sort can be fully understood only in terms of the social context in which it developed and in which it functions.

It is in connection with this postulate that the anthropologists have made the most valuable contributions to psychological thinking. As was noted under Postulate 3, general psychology frequently takes the social context for granted, simply because it is common to both psychologists and students. In the fields of personality and social psychology, however, it cannot be tacitly assumed, but must be examined. The result is that "human nature" is shown not to be more or less the same the world over, but that the human individual is a complex "organism-in-society." Hence, the current interest in attitudes, for example, as one area in which the dependence of the individual upon his society is most readily (although not easily!) studied.

Postulate 6 may not seem to harmonize with Postulate 1, but there is no actual contradiction. In Postulate 6 we are dealing with activity in progress, so to speak, and the behavior that develops during the life history; in Postulate 1, we dealt primarily with the starting points of behavior, which are quite rudimentary and largely undetermined in their ultimate direction and expression.

Psychologists nowadays have learned to be cautious in ascribing to "human nature" any aspect of behavior without at least taking account of the implications of Postulate 6. Indeed, they have criticized Freudian psychoanalysis in precisely these terms. They beware, now, lest ethnocentrism of any variety befuddle their thinking.

Postulate 7: *Objectivity.* No interpretation of human behavior can be fully accepted unless it is founded on fact substantiated by scientific evidence.

No particular comment is necessary. Today's psychologists, keenly aware of the difficulties involved in the study of behavior, and strongly influenced by the standards of other sciences (more so, sometimes, than scientists in those other fields), think in careful methodological and evaluative terms. They beware just as much (more, in fact) of egocentrism as of ethnocentrism. True, there is still a long way to go, but every modern reputable psychologist receives intensive training in terms of Postulate 7. The behaviorists are especially to be thanked for this trend.

Postulate 8: *Finality.* In the study of human behavior, no interpretation, conclusion, or law is necessarily final, but is given in the light of the

best present knowledge; allowance must be made for the possible operation of unknown or uncontrolled variables, and for other possible limitations of knowledge or outlook.

This statement, also, requires little comment, since it is no doubt basic to all fields of inquiry. It means that psychologists, on the average, do not pretend to have all the answers. They continually strive to improve their methods and to push further the frontier of our understanding of man; they continually examine and retest their existing experimental results and theoretical formulations. Psychologists are still hazarding hypotheses and conducting investigations. Psychologists are no more complacent, arbitrary, or pedantic than other scientists, who also retain a sense of curiosity, of criticism. The realm of understanding has not yet been conclusively defined.

Perhaps more than any other, Postulate 8 is the reason for writing this article. It reflects the fact that in the history of psychology we have often had to change our interpretations of behavior as new facts and theories emerged. Thus, it becomes valuable, as in the present instance, to formulate our position as of the present. But it is not necessarily an absolute or final formulation. The psychology of the future may force us to revise or reject our basic postulates or to add new ones.

Postulate 9: *Modification.* Human behavior is not static; the individual develops and his behavior is modified and remains modifiable as long as life continues.

Modern psychology, with its intense emphasis on learning, development, and activity, would not be recognizable to psychologists of fifty or sixty years ago. No modern psychologist thinks of a human being as a static, largely unvarying thing, to be described in terms of sensations, images, feelings, and ideas. Instincts, as universal, fixed determinants of behavior, have been rejected. Even if certain innate forces do exist, it is now being shown, even in lower animals, that they are modifiable when conditions change.

Similarly, the relationship between childhood, adolescence, and adulthood is now continually emphasized and studied. Nor is prenatal development ignored. Modern psychology is no longer that of an adult (primarily male), of twenty to forty-five years of age, who resembles a mounted specimen in a museum showcase. Rather, man is studied in terms of continuous growth and function, literally from conception to death. Thus, even when adult behavior is the focus of attention, it is not considered to be rigid, and senescence will become an important field of research in the future.

Postulate 10: *Measurement.* Potentially, all human behavior can be measured and described.

This postulate implies that, however mysterious some aspects of human behavior may be, they are ultimately understandable in scientific terms. All too frequently, the modern psychologist must qualify his interpretations with the statement "We are not yet sure," or "That has not yet been determined." However, most psychologists are certain that sooner or later investigation will uncover the still-hidden facts of behavior.

Postulate 11: *Diverse views.* Human behavior may be, and has been, interpreted from different viewpoints, no one of which is necessarily right or wrong, and all of which may contribute to complete understanding.

Postulate 11 is the modern eclectic viewpoint and reflects a moderate approach to the assertions and theories of different "schools." Glance through any standard textbook on psychology or the bibliography of almost any article that deals with a general problem. It will be apparent that all the "schools" are represented. In the former case, the history of the great controversies is presented in disguised form. For example, the chapter on motivation and personality will reflect psychoanalytic contributions; the chapter on learning will probably owe much to behaviorism; the chapter on sensory processes can be traced, in part, to Titchener; and the chapter on perception will lean heavily on gestalt psychology. Even William James will enter into the discussion, at least in the chapter on emotion. It is no wonder that the modern psychologist thinks in terms of Postulate 11. He is historically determined, as E. G. Boring has sagely pointed out.

Postulate 12: *Fractionation.* If, for convenience, aspects, or parts, of human behavior are studied separately (or if apparently divisible units are analyzed), it cannot be fully understood or described in terms of these elements, but must finally be viewed as a whole organization.

The above statement sums up the most significant contribution of the gestaltists to psychological thinking. So insistent have they been, and so articulate, that psychologists of the present think less in terms of units (such as sensations, images, and conditioned reflexes) and more in terms of patterns and sequences. No aspect of behavior is seen as separate and distinct from other aspects; the activity in progress at the moment is viewed in relation to the total state of the organism, as organized on the basis of past experience, other current functions, and even, when necessary, foresight or planning for the future.

Although the gestaltists have dealt most extensively with visual perceptual processes, psychologists have been markedly influenced by them in other connections also. Referring once more to standard texts, one would find in nearly every chapter sections devoted to the relationships between diverse phenomena, functions, and activities. There would be found many cautions against fractionating behavior, toward the end of showing the total individual, not as a sum of elements, or a composition of units, but as a complex interrelated organization of continuous patterns, functions, and processes.

Postulate 13: *Dynamics.* All human behavior has a cause.

Freud contributed to psychology, as much as anything else, the important viewpoint that mental processes, emotional impulses—indeed all activities—are dynamically caused, that there is a reason for their occurrence. No matter how irrelevant, accidental, or inexplicable a person's behavior may seem, it does not occur simply at random, but stems from some actual, though perhaps concealed, source within the individual. Psychologists now appear to accept this idea.

Postulate 13 may, perhaps, be regarded as a correlate on the psychological side of Postulate [illegible]

IN THE foregoing discussion I have endeavored to set forth the broad outlines of the current psychological viewpoint, with some measure of historical perspective. The era of "schools" is about over in psychology, as Keller observes. The eclectic psychologist no longer troubles himself very much with them, save in historical surveys, and the standard textbooks represent fairly well a synthesis of them. I believe that the average student now gets substantially the viewpoint presented above.

This article is designed to present the broad framework of this thinking for the consideration of other scientists. All too often there is doubt as to what it is, and whether any progress has been made. Actually, psychology has shown a healthy development, as a result of a vast and lively body of experimentation and systematic thinking. Nor has this growth displayed the slightest sign of diminishing return. On the contrary, these paragraphs may be regarded as the discussion of a temporary condition, really the bare minimum of conceptualization in psychology as it exists today.

Photographic Exhibitions

Entries in the *Second Annual International Photography-in-Science Salon* will be received by The Scientific Monthly July 26–August 16, 1948, inclusive. They will be judged on August 21, and those accepted will be shown in the Natural History Building, U. S. National Museum, Washington, September 1–21. Already booked through April 1949, showings after that date may be arranged.

The Biological Photographic Association will hold an exhibition in Philadelphia, September 8–10, in Houston Hall, University of Pennsylvania, at which prints, color transparencies, and motion pictures in the field of biological photography will be shown. There will also be organized symposia and demonstrations.

THE IMPORTANCE OF THE INDIVIDUAL IN HUMAN EVOLUTION*

HAROLD F. BLUM

Dr. Blum (Ph.D., California, 1927), who has taught physiology at Oregon, Harvard Medical School, and California, was associated during the war with the Naval Medical Research Institute, Bethesda, Maryland. He is now a physiologist at the National Cancer Institute and visiting lecturer in the Department of Biology at Princeton University. The article below was written while Dr. Blum was a Guggenheim Fellow.

For man is enabled through his moral faculties to keep with an unchanged body in harmony with a changing universe.—Alfred Russell Wallace (quoted by Charles Darwin).

MAN'S behavior is a part of man himself, and his history and culture, which have helped to shape this behavior, are parts of his evolution. This premise must, I believe, be accepted before attempting to view human evolution in its entirety. For purposes of analysis, one may choose to treat human evolution at a purely biological level, but in doing so it should be recognized that a part of the total picture is being omitted. Man's influence upon evolution in general, resulting from the environmental changes he has created on the surface of the earth, is a factor of rapidly increasing importance. Can we neglect this factor in evolution even at the biological level? And, conversely, it should be admitted that the factors that account for organic evolution may not be sufficient to explain the whole of human evolution in the above broad sense. If I view the matter correctly, misconceptions occur not infrequently because of failure adequately to analyze the factors involved; and it is with the hope of dispelling some of these misconceptions that this article has been written, not to introduce any novel biological principle.

What I shall emphasize is that certain main principles underlying the behavioral aspect of human evolution are not only different from those upon which organic evolution is based, but that they need to be regarded, quantitatively, in a quite different manner. In the end we shall arrive, I believe, at a point of view harmonious both with the ideas of the evolutionary biologist and of those more immediately concerned with the humanities.

Let us first examine very briefly the essence of the neo-Darwinian concept of evolution. Essentially, it involves variation by genetic mutation and recombination, and natural selection by elimination of the least-fitted to the environment and isolation of the old from the new. Strictly, the phenotype—the individual as outwardly expressed—is the point of impingement of natural selection; but it is the genotype—the genetic individual—that must be "selected" in order to accomplish evolutionary change. I shall assume without reservation that these principles apply to the evolution of man both in the past and in the present, rejecting the Lamarckian concept of inheritance of acquired characters in any biological sense. This is the point of view generally accepted by biologists, and I will not be concerned herein with the question of whether it is adequate to describe all organic evolution, which question has relatively little bearing upon my thesis.

In rejecting the concept of inheritance of characters acquired within the lifetime of the individual, it may be pointed out that the history of attempts to demonstrate by experiment the existence of such a mechanism has been one of consistent failure. Even more pertinent is the failure to formulate a reasonable theoretical explanation for such inheritance of genetic characters. The concept has always been a more or less tempting one because by accepting it all the difficulties of explaining adaptation to environment are removed—by definition, as it were. Charles Darwin himself—possibly in despair of any other explanation—tacitly accepted the concept in formulating his theory of "pangenesis," which he developed subsequent to the appearance of the *Origin of Species* in 1859, and set forth in his *Descent of Man* in 1871. Pangenesis was quite widely accepted until it was rendered untenable by the development of modern genetic theory. It is of historical interest that the concept of inheritance of acquired characters, to which modern Darwinism is directly opposed, flourished for a considerable period under the aegis of Darwin himself, and subsequent to his formulation of the concept of natural selection upon which his fame deservedly rests.

The individual. In comparing the individual of a species with his species mates, it is usual to study one aspect at a time. If this aspect is measured,

* From an address presented before Section L, AAAS, December 1946.

it is always found to vary, within limits, from individual to individual. Thus, measurements of a given aspect X—which may be the height of a tree or the weight of a man—are found to distribute themselves among the population in a manner something like the curve A in Figure 1. For the greater proportion of the individuals in the population, aspect X has a value near the mean, the proportion decreasing as we move away from the mean in either direction, until the extremes are represented by only a very few individuals.

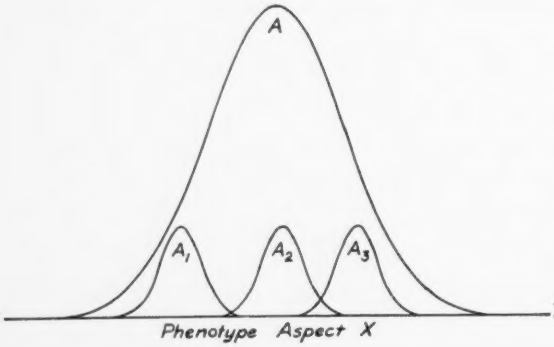

Fig. 1. Scheme to illustrate the distribution of a given aspect X, within a species, A, and within pure genetic lines isolated from that species, A_1, A_2, A_3. The curve drawn is that for a normal distribution and may be regarded as the "ideal," whereas those distributions actually found in nature are likely to be skewed or otherwise modified from this basic form; but for simplicity we may assume that this distribution is characteristic of any aspect with which we may be concerned.

If individuals from one extreme of such a distribution are selected and bred together, and selection repeated in like manner for the offspring, a pure breeding line may be obtained which will show less variation of the individuals with regard to this aspect. Such a pure line might have a distribution like that indicated by curve A_1 in Figure 1. The pure line is established within a few generations, after which further selection within this line will not result in further narrowing of the distribution, and it is to be concluded that this occurs when all the individuals with the pure line have identical genetic make-up, at least as regards the aspect under study. By this process of selection and inbreeding, it may be possible to obtain several pure lines, as indicated by the curves A_1, A_2, and A_3 in Figure 1. Within these pure lines, genetic uniformity has been arrived at, but not complete uniformity of the group as outwardly expressed by the individuals. At least some of the individual variation may be due to inequality of environment, but this of course has no influence on the genetic make-up of the individuals. Since its demonstration by Johannsen early in the century, such isolation of pure lines has been carried out in many species of plants and animals. No such experiment has been performed on man, of course, but there is every reason to expect the same kind of result.

There will occur rarely among the population of a pure line an individual with a somewhat different genetic make-up as regards a particular aspect. This is called a mutant, or sport. If a population composed of the offspring of such a mutant individual could be immediately isolated as a pure line, and compared to the original pure line from which it arose, the distribution of aspect X in the two pure lines might be respresented by the curves A_1 and B in Figure 2, this aspect being distributed about different means in the two cases. Actually, it is not possible to isolate the mutant line immediately, but by conscious selection on the part of man this could be accomplished within relatively few generations.

In nature, the isolation of such a line would take longer, but if the mutant line were in some way better fitted to the prevailing environment than the original line, its proportion in the population would tend to increase, and it might eventually dominate, or under proper circumstances eliminate its predecessor. This represents the process known as natural selection, and such selection of mutant forms is the basic mechanism by which evolution in the neo-Darwinian sense is accomplished. (These and some other of the statements regarding mutation and natural selection have been oversimplified here for the sake of brevity.)

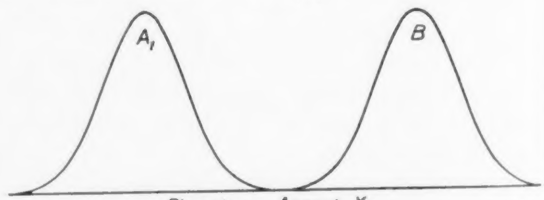

Fig. 2. Scheme to illustrate the distribution of aspect X in a pure genetic line, A_1, and a mutant therefrom, B.

Thus far the discussion has, for the sake of simplicity, dealt with one aspect only. To describe a population completely would require the description of a myriad of separate aspects, and this could only be accomplished with a three-dimensional diagram. To make such a diagram one might place face to face a great number of sheets of paper each having the shape of, say, the area under curve A_1 in Figure 2, each sheet being supposed to represent the distribution of a different aspect, closely related aspects being placed next each other.

In this way might be constructed a block shaped as represented in Figure 3, which would describe the population completely as regards the aspects in-

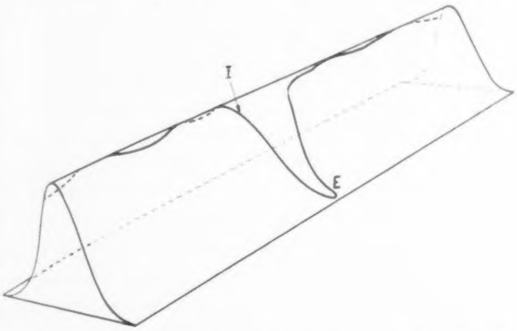

Fig. 3. Three-dimensional scheme to represent the distribution of many aspects of a population. Any cross section of the block may be thought of as representing a single aspect. Thus, the whole block describes a population. The diagram might be improved by having the block curved upon itself in a circle to represent an endless succession of aspects, those intimately related always lying adjacent.

cluded. The block might be thought of as describing the whole of the population on which the aspects were measured, and then any individual from this population could be described by a contour line on the surface of the block, such as that indicated by I in Figure 3. For most individuals, this contour might be expected to follow the ridge of the block fairly closely throughout most of its course; that is, it would describe the individual as being close to the mean in most aspects. In some aspects, however, the individual might be close to an extreme, as indicated by the sharp swing of the contour represented at E.

Cultural evolution as contrasted to biological evolution. Let us now assume, for the purpose of developing our argument, that the block shown in Figure 3 presents a pure line of human beings, and that the particular aspect that is represented by the cross section in which E falls is something we shall call "inventiveness." If an individual displaying an extreme of this aspect mates with another individual who also displays an extreme of this aspect, the offspring of the mating would have no greater chance, biologically, of showing extreme inventiveness than the offspring of any other mating within the whole population. Remember that this is said with reference only to the case of a hypothetical pure line, for this need not be true for mating within the real population where the two individuals with extreme inventiveness might actually belong to genotypes superior in this aspect, although the chances of this happening would probably not be very great for any single mating.

On the other hand, if, under any circumstances of heredity, an individual actually creates an invention which becomes accepted by, and so has an influence upon, the culture of the population as a whole, the individual will have had a direct effect on human culture and will have contributed to cultural evolution. The same may be said, of course, for an abstract concept which becomes a part of human culture and which may have more far-reaching consequences than a material invention. In a cultural sense, the group as a whole has inherited a facet of culture directly from the individual, and with regard to this particular cultural facet the individual enjoying an extreme of a certain aspect may be more important than the group as a whole. This is a very different matter from genetic inheritance, and evolution in the strictly biological sense, where the individual displaying the extreme of any aspect is of no more importance than any other member of the genotype. On the other hand, the ability to transmit ideas and concepts from one generation to another is a form of "inheritance" which has no parallel in a strictly genetic sense.

Human evolution, in a biological sense, is no doubt going on continually. There is no such thing as a pure line of human population, and hence selection of characters having survival value of one kind or another may go on to a certain extent. Since new mutants may arise from time to time that have survival value, a certain amount of evolution toward better adaptation to the environment may be expected, but this is an extremely slow process in terms of human history. Mutations involving some aspects probably occur today about as rapidly as once per hundred thousand or million of the population per generation, but only a few of these can be expected to have survival value; probably most will be detrimental. For the most part, those that survive can be expected to provide only relatively slight alterations of type. The effect of these mutations on the character of the population as a whole must require a long time to manifest itself even under the most favorable circumstances.

Selection by man himself, if we could trust his knowledge and judgment, might cause slow improvement along special lines. That the eugenist's attempt to eliminate from the population certain clearly defined detrimental inherited characters may have its dangers when his methods are employed by irresponsible groups has, however, had recent frightening demonstration that need not be dwelt upon. And under the best handling this must be a slow method of improvement, as well as one to be approached with caution.

The idea that human evolution in a desirable

direction is favored by encouraging severe competition between individuals of the human population to accomplish "survival of the fittest" is fallacious because it does not recognize the true implications of natural selection. ("Survival of the fittest" was Herbert Spencer's more dramatic synonym for Darwin's "natural selection.") Competition between species may eliminate the weaker species; competition within a species might eliminate the species itself. In its evolutionary effect, natural selection is not concerned with the individual, but only with its genotype:

> So careful of the type she seems,
> So careless of the single life.

In contrast, the individual would seem to play a dominant role as regards cultural evolution. To be sure, any contribution to the culture of the group must be taken up by the group as a whole in order to have any extended effect thereon, and whether a new invention or idea is taken up may depend upon whether circumstances are favorable to its reception; these circumstances may involve a complex of factors. But it would seem generally to require the contribution of a single individual to initiate such a cultural change. We are here concerned only with human culture, but we may speculate as to the possible role of the individual in the evolution of learned behavior in lower animals. This seems to have received relatively little attention, yet it might be subject to experimental study.

To return to human evolution, I should like to quote some statements from the *Descent of Man* (London: John Murray, Chapt. V, 161). All are from the same paragraph, but have been rearranged to separate two lines of thought:

> Now if some one man in a tribe, more sagacious than the others, invented a new snare or weapon, or other means of attack or defense, the plainest self-interest, without the assistance of much reasoning power, would prompt the other members to imitate him; and all would thus profit . . . . if the new invention were an important one, the tribe would increase in number, spread, and supplant other tribes.

This, I think, seems to be good reasoning, although Darwin may have underestimated the inertia of the traditional mind in matters of common defense as well as in many others. With certain other statements we cannot so readily agree; but I shall quote them for their historical interest, and because they may be significant in regard to ideas that have to a certain extent remained current.

> The habitual practice of each new art must likewise in some slight degree strengthen the intellect. If such men left children to inherit their mental superiority, the chance of the birth of still more ingenious members would be somewhat better, . . . Even if they left no children, the tribe would still include their blood relations. . . .

Clearly there is reference here to the inheritance of acquired characters. We need to remember that when he wrote this Darwin was thinking in terms of "pangenesis," which implied such inheritance. The theory of pangenesis dominated thought for about two decades, and I believe we still have in our social thinking evidence of the belief in a gradual improvement of the genetic character of the population in the way implied in the above quotation. Of course, no mechanism for such improvement has been demonstrated, and modern evidence is to the contrary.

So far as the selection of individuals is concerned, it would have no evolutionary effect unless the individuals selected were genetically superior. History seems to indicate that brilliant and effective individuals have all too often left offspring of much less talent and little historical significance—with meaning we may ask, "Where is the doughty Charlemagne?" On the other hand, certain families have contained a high proportion of effective individuals, although biological inheritance is certainly not the sole factor operating in such cases. Both facts are consistent with what has been said herein, and both should be given their proper weight when we consider human evolution.

In summary, then, genetically speaking, the individual as such is of no great evolutionary importance unless he be the bearer of a mutant gene, and even then the importance of the mutation may not manifest itself in the population as a whole for generations. In cultural evolution, on the other hand, the individual would seem to hold a position of great importance. The mechanisms in the two instances are so different that they can hardly be discussed in the same terms. In fact, I think some biologists may object strongly to the application of the term evolution in the second case, and if it should lead to confusion of the two mechanisms I would agree, emphatically. I think, however, that there is much to be gained by treating the cultural aspect as a part of human evolution, but above all things we need to understand and to separate clearly in our minds the underlying processes.

IS MATHEMATICS AN EXACT SCIENCE?

N. A. COURT

Born in Warsaw at a time when it was a part of Russia, Professor Court received the degree of Docteur en sciences physiques et mathématiques *at the University of Ghent in 1911. In that year he came to the United States to teach mathematics and since 1916 has been in the Department of Mathematics at the University of Oklahoma. Dr. Court is the author of* College Geometry *and* Modern Pure Solid Geometry.

THE famous eighteenth-century *Encyclopédie Métodique* gave the following definition of mathematics: *C'est la science qui a pour objet les propriétés de la grandeur en tant qu'elle est calculable ou mesurable.* Precise, concise, definite, and simple. This was in 1787. Even though this definition was adequate for the time, it was not destined to remain so very long. Two decades earlier Gaspar Monge had invented descriptive geometry. He did not publish his results until 1795, because for over a quarter of a century the French High Command considered descriptive geometry its own private military secret. Monge's invention led his pupils to the creation of projective geometry, a branch of mathematics that does not deal with magnitude. The quantitative conception of mathematics thus became obsolete. Many efforts have been made since to find a definition that would embrace all of mathematics. The enormous growth of the science during the past century and a half, and the inclusion of such branches as the theory of groups, topology, and symbolic logic, rendered all such attempts unsatisfactory. The hopeless task was finally given up in favor of simply saying that mathematics is what mathematicians are doing.

How do mathematicians acquit themselves of the heavy responsibility that such a definition puts upon their shoulders? They have the advantage that they start out with a great amount of credit. To the layman mathematics is synonymous with exactness, nay, with certainty. Mathematics is precise, mathematics proves all the assertions it makes, all the propositions it advances. And books written by mathematicians seem to bear out the layman's opinion about the authors. These books seem to be written with complete detachment and strict objectivity. There is not a single exclamation point to be found on any of their pages, except when it is used as a symbol for a factorial. But do mathematicians actually do their work with that Olympic impartiality that the final product seems to exhibit?

Ask any mathematician worth his chalk why he spends so much time and effort on his research, and he will almost invariably tell you—and quite truthfully so—that he does it because he finds it very interesting, because he loves to do it, because to him it is a most exciting adventure. Sentimental reasons all. But is sentiment a reliable partner of objectivity?

Every active mathematician will readily agree that he is trying by his efforts to promote and advance the science of his choice. There is no doubt that he is telling the truth and that he is quite sincere about it. But is it "the whole truth and nothing but the truth"? If it were, if the mathematician were interested in the promotion of his science in a purely objective way, it would make no difference to him whether it was A or B that took a given forward step, as long as the advance had been accomplished. But this is not the case, as is abundantly proved by the historically famous, and disgraceful, controversies over priority rights of mathematical inventions. The Newton-Leibnitz quarrel over the invention of the calculus was just as bitter as it was harmful. It actually hindered the progress of the calculus in Britain for over a century.

The dispute between Poncelet and Gergonne as to who was the rightful owner of the title to the invention of the principle of duality may have yielded in scope to the Newton-Leibnitz controversy, but it was fully as acrimonious, if not worse. One could cite the quarrel between Descartes and Fermat, between Legendre and Gauss, and so on and on, *ad nauseam.* Cardan obtained from Tartaglia the solution of the cubic equation under oath of secrecy and then not only published the solution, but claimed it as his own. Our methods may not be as crude, but we are as jealous of our priority rights now as anybody ever was. Editors seem to think that priority claims are established by the date a given article reaches their desk, and publish this date as a part of the article. Perhaps whether it was A or B that made a contribution may not be of so much moment, but whether it was *I* or not *I* is of tremendous importance. The sublime indifference toward public acclaim exhibited by a Fermat does not seem to be of this planet. It may be argued that mathematicians as a rule get little else for their labors; they are therefore at least entitled

to the honor and recognition their accomplishments can bring them. This is true enough. But it is a weak argument in favor of the supposed detachment and objectivity with which mathematicians view their work.

There are cases on record when mathematicians were reluctant to publish the results of their findings, their reticence motivated by their solicitude for their science. When the researches of the Pythagoreans brought them face to face with irrational numbers, they were overwhelmed by their discovery. It contradicted the fundamental tenet of their philosophy that everything is (rational) number. The surest way out was to make of this troublesome result a professional secret and to induce the gods to destroy anyone who would dare to divulge to the lay crowd the exclusive wisdom with which only the initiates could be trusted.

We have a similar example in modern times. Gauss was in possession of non-Euclidian geometry ahead of both Lobachevsky and Bolyai, but he was loath to publish his results. He feared that such an unorthodox discovery might undermine the faith of the young in the validity of mathematics in general.

The judgment of both the Pythagoreans and of Gauss as to the effect of their discoveries upon the development of mathematics was totally wrong. But this is here quite beside the point. What is important to note in this connection is that the concealing of the truth is hardly the proper method to inspire confidence in the exactness of the science one is trying to promote.

For men who are supposedly dealing with an exact science, the number of mistakes mathematicians make is both puzzling and disconcerting. A few years ago the Belgian mathematician Lecat published a collection of *Erreurs des Mathématiciens.* The list of names mentioned looks like nothing less than a "Who's Who in Mathematics." Henri Poincaré was awarded the Nobel prize for a paper that had a serious mistake in it. He detected the error himself while his paper was in the process of being published, but it was too late to remedy the situation, and the King of Sweden formally conferred upon the author the Nobel prize for a paper that was wrong.

In an exact science it should be easy to evaluate the merits of a paper, and experts in the profession should be able to decide which of several solutions of the same problem is the correct one. But this is only too often not the case. Here is one example, of many that could be quoted. In a paper *Fourier's series,* recently published by the Mathematical Association of America, R. E. Langer relates a controversy participated in by d'Alembert, Euler, and Daniel Bernoulli. Each of these luminaries wrote a paper on the problem of vibrating strings. The three-cornered polemic lasted more than a decade. The only point of agreement that emerged clearly was that there was always a two-to-one majority that the third party was wrong. Human, all too human. But where does the exact science come in?

In spite of all these foibles, mathematicians mount a vigilant and jealous guard over the exactness of their science and are not a bit sparing of one another when the impeccability of their science comes into question.

The invention of the calculus provoked a flood of criticism as to the mathematical and logical soundness of the new doctrine. Neither Newton nor Leibnitz was quite convinced that the reproaches were groundless, but they found no way of disposing of them. Leonard Euler (1707–83), their most distinguished immediate continuator, paid still less attention to this controversy. He used his great gifts to expand and enrich the work of his illustrious mentors, and his unerring instinct for what was right kept him firmly on the straight path. However, Lagrange (1736–1813), a younger contemporary of Euler, did not share the faith of the courtier of the czars of Russia in the formalism of mathematics. In Lagrange's estimation Euler's calculus "did not make sense."

The mathematical analysis bequeathed by the eighteenth century appeared to the mathematicians of the early nineteenth century to be a structure totally devoid of any foundation. Under the leadership of A. L. Cauchy (1789–1857) they undertook to provide analysis with underpinnings solid enough to render this branch of mathematics impervious to the most exacting criticism and at the same time to safeguard the results of mathematical analysis from all possible errors.

Thus came into being the school of rigor of the first half of the nineteenth century. It accomplished a great deal, but its achievements were anything but final. The second half of the nineteenth century set new goals for rigor. An attempt was made to "arithmetize" mathematical analysis. Dedekind produced his theory of irrational numbers, Georg Cantor the theory of point sets, and so on. And the quest for rigor is still on the march. What satisfies the most rigid requirements of one generation of mathematicians seems totally inadequate for the next. E. H. Moore (1862–1932), for many years professor of analysis at the University of Chicago, expressed this in the apt adaptation of a biblical phrase: "Sufficient unto the day is the

rigor thereof." It would seem, however, that mathematical rigor is a very elusive thing. The harder it is pursued, the more adroitly it evades the pursuer. In spite of all the advances that the nineteenth century contributed toward mathematical rigor, the mathematicians of the present generation feel that they are more "up in the air" than any other generation ever was.

As a textbook Euclid's *Elements* has no rival, not only in mathematics, but in any other subject. More people over more centuries have learned their geometry from that book than have learned any other subject from any other single book, with the exception of the Bible. And yet this is not the greatest of the merits of the book. The great role that this book played in the cultural history of mankind is due to the fact that Euclid's *Elements* was the first model of a deductive science. Euclid begins by defining the entities he is going to consider: point, line, angle, etc. Then he lines up his axioms and his postulates, i.e., those propositions that he accepts as valid on account of their plausibility or "obviousness." All the propositions that follow are derived from those assumed by pure reasoning, according to the strict precepts of logic. For some two thousand years there was nothing that approached Euclid's model in perfection.

It is a queer irony of our intellectual history that it is precisely this perfection of Euclid's geometry that inspired the invention of non-Euclidean geometry. All through the ages students of geometry felt that Euclid's parallel postulate was not sufficiently obvious and should be proved. But the many and various attempts to prove it failed. In the first half of the nineteenth century Lobachevsky and Bolyai, following Euclid's model, each independently constructed a non-Euclidean geometry by assuming that Euclid's parallel postulate is not valid. Each of them pushed his geometry far enough ahead to convince the most skeptical that their systems are quite coherent and not likely to run into inconsistencies. All doubt on this score was finally dispelled when it was shown that the Lobachevskian plane non-Euclidean geometry may be interpreted as Euclidean geometry on a pseudo sphere.

The non-Euclidean geometries rendered Euclid's parallel postulate, if anything, even less obvious. Still Euclid succeeded in constructing his elements in spite of this deficiency. From this there was only one step to the conclusion that the logical coherence of Euclid's *Elements* is in no wise dependent upon the obviousness of its postulates, and that it should be possible to build a consistent geometry with a set of postulates that would lay no claim to obviousness whatever.

The basic entities of Euclid's great work fared no better than his axioms. It all started with the "principle of duality," to which allusion has already been made. This principle asserts that if in any valid proposition of plane projective geometry the words "point" and "line" are interchanged, the resulting proposition is also valid. This astounding discovery inevitably led to a strange conclusion, namely, that the nature of the basic entities to which the basic postulates of a deductive science are applied is quite immaterial. In fact, these entities need not have any meaning of their own. Their relation to each other is determined by the postulates that are applied to them, and that relation is all that matters.

On these foundations was built the "formalist school" of mathematics, of which David Hilbert (1862–1943) was the leading exponent, the high priest of the cult. There was, however, a bothersome fly in the ointment. In fact there were two such flies. If postulates for a mathematical science, for example, geometry, are set down arbitrarily, and if the entities to which they are applied are devoid of meaning, what relation does such a geometry bear to the physical world? Richard Courant (1888–), a former colleague of Hilbert, says in the preface to his "*What is Mathematics?*" (1941, written in collaboration with Herbert Robbins) that such a doctrine "is a serious threat to the very life of science," that "such Mathematics could not attract any intelligent person." The formalists, however, made short shrift of objections of this kind as long as they could feel that their science remained logically without a blemish. On that ground they were undeniably right. But it was not so easy to kill the other fly, for nothing less was involved there than the logical foundation of the formalist science.

The "obviousness" of Euclid's basic propositions had reference to the fact that these propositions are extracted from our daily experience and are realized, somewhat crudely, in the world that surrounds us: they are thus consistent with one another. If the postulates are taken arbitrarily, if they have no intuitive connotation, what guaranty is there that they are logically consistent? Without a proof of the consistency of the postulates the whole edifice is worthless. The formalists realized that no less than their bitterest critics. Hilbert made heroic efforts to find such a proof. He failed. And there the matter rests, except that it has been proved to the satisfaction of those most competent to judge that, within the framework of a given

formalist science, it is not possible to find a proof that science is consistent. If a proof of consistency for a formalist science is to be produced, it must come from outside that science. This proposition is due to K. Goedel.

The formalist school of thought in mathematics takes logic for granted. To this logic it adds an arbitrary set of entities—"undefined terms"—and an arbitrary set of postulates—"unproved propositions." It is then in possession of all the necessary tools and materials for the building of the proposed branch of mathematics.

Another school of thought came to the conclusion that the formalists are extravagant: they require too much. Logic alone is perfectly sufficient for the erection of the entire edifice of mathematics. Not the old verbal logic, but logic reduced to a set of symbols, after the manner of algebra. By means of this "symbolic logic," to give it its proper name, all mathematical entities, including the integers themselves, can be obtained by purely logical constructions. This philosophy of mathematics culminated in the three-volume work *Principia Mathematica* (1910–13), by A. N. Whitehead and Bertrand Russell. This was an extremely ambitious undertaking, undoubtedly one of the greatest intellectual enterprises of all time. It was hailed with great enthusiasm in many quarters, especially in England and in the United States. Helping hands came forward to render the great work still greater.

But the *Principia* began to suffer from the same malaise as Cantor's theory of point sets, as the infinite processes put to work to provide a logical foundation for the mathematical continuum. Paradoxes and antinomies came to light that were very embarrassing. Some of the fundamental assumptions of the *Principia* introduced for the express purpose of warding off paradoxes were found to be questionable and finally rejected. It was not long before the *Principia Mathematica* was reduced to the status of one more contender for the honor of being the custodian of the foundations of mathematics, under the name of "logicalism."

Among the critics of the *Principia* were the French intuitionists: Borel, Lebesgue, and others. But the greatest challenge of this work came from members of the Dutch school, called by Abraham A. Fraenkel the "Neointuitionists." This school, under the leadership of L. E. J. Brouwer (1882–), put the *Principia* upside down. Not only did they reject the idea that mathematics can be derived from logic, they denied logic any autonomous existence. Logic, according to the intuitionists, is not a science but a technique derived from science to facilitate the study of the science. Furthermore, Brouwer boldly questions the validity of the basic processes of our generally accepted logic. He rejects the law of the excluded middle, i.e., that a proposition is necessarily either true or not true. It may be neither, for there may be no sufficient information to decide the question.

As an illustration of what is meant by Brouwer's negation of the law of the excluded middle, let us consider the example given by Abraham A. Fraenkel (*Scripta Mathematica,* **13**, Nos. 1-2, 1947). The fractional part of the number π has been computed for many hundreds of places, and many more such places could now be computed with much less labor than before, by means of the new electrical calculators. Is there a place in the long row of numbers where the digit 7 occurs seven times in a row? There is no such place in that part of the fraction that is known at present, and we cannot tell whether it will or will not occur if new digits of that fraction are computed.

Now let us consider the real number R which starts out as 0.333333 and every other digit of this decimal fraction is a 3, except that if the nth digit of the fractional part of π is a 7 followed by six more digits 7, we will take for the nth digit of R the digit 2, if n is odd, and the digit 4, if n is even. The digits of R are thus perfectly defined as far as the digits of the fractional part of π are known. But we cannot tell whether R is equal to $1/3$, smaller than $1/3$, or greater than $1/3$.

Is the famous saying "You cannot fool all the people all the time" true or false? Perhaps it is true. But it is conceivable that a man publicly perpetrated a hoax or a lie that remained undetected during his lifetime and that he took his secret with him into his grave. Then the proposition would, of course, be wrong, but we would have no way of proving it. If the man wrote a confession, sealed it, and ordered his heirs to open it on the one-hundredth anniversary of his death, then we shall find out on that day that our proposition is false. But at present the proposition is neither true nor false. Hitler was quite certain that the proposition is false. Witness his principle of "the big lie."

"Francis Bacon is the author of the so-called Shakespearean plays." Is the proposition true or false?

Things did not become any smoother for any of the contending schools of thought when the Polish logician Lukasiewicz raised the question why logic should be limited to only two alternatives, two values: true and false. He proposed a new logic which admits of three alternatives, a three-valued logic. Now *ce n'est que le premier pas qui coûte.* If logic can be three-valued, why can it not be

four-valued, indeed, why not n-valued? There is no reason, however, to stop there. Why must the values of logic be a finite whole number? We might as well have a logic with a continuous number of values—such proposals have been advanced.

That unshakably solid rock of classical logic simply slipped away from under the mathematical edifice, and the whole structure is now "on the rocks." As mathematicians put it, their science is at present passing through a "crisis." It has been in this state, roughly, since the beginning of the present century. What connection, if any, is there between this crisis, and the social and political turmoil in the throes of which suffering mankind has been laboring during the same period of time? This is not the time nor the place to consider this question, but so far as mathematics is concerned, you need not be overly alarmed. Mathematics is not going to the dogs.

Mathematics has two aspects: On the one hand, it is a description of a segment of the world we live in and it furnishes tools for nonmathematicians to describe other segments of that world. This might be called the "functional" part of mathematics. The other part of mathematics deals with its foundations and may be said to be largely philosophical. Of course the two parts are not unrelated. The study of the foundations of mathematics decides how far the mathematical processes may be carried out before they reach the limits of their validity. Fortunately, whatever these limits may be, there is ample room for mathematical activity long before those limits are reached. As a matter of fact, most of the active mathematicians are little concerned about those foundations. At least they do not allow those problems to interfere with their activities as investigators. More than that, even those mathematicians who take a direct part in the debate regarding the logical validity of their science manage to obtain very valuable results in their own special field of investigation that have little relation to those theoretical discussions.

But what about the crisis itself? It would, of course, be foolhardy for anyone to try to predict at present where the crisis leads to and how it will end. What may be said, however, with perfect safety is that mathematics will emerge from it enriched and invigorated, to continue the work it has been so successfully carrying on up to now. Some of the benefits are already becoming apparent.

The crisis is having a sobering effect upon mathematicians. The ancient Greek mathematicians used to say that their gods "geometrized." In recent times the God of the English astronomer Jeans was a "pure mathematician." These views reflected the feeling, never formulated, that some mathematicians had that, through their science, they were creating absolute truths, that their profession is the custodian of some eternal verities. During the latter part of the nineteenth century there was much talk about the possible inhabitants of Mars. The desire arose to find a way of getting in contact with those hypothetical creatures through light signals. Some mathematicians came forward with the suggestion that the signal should be a gigantic right triangle built somewhere in the Sahara Desert, the assumption being that if there are intelligent inhabitants on Mars, such a signal will convey to them the idea that there are intelligent inhabitants on the earth, since the Pythagorean theorem is a universal truth.

Mathematics, in spite of great achievements, in spite of the prestige it enjoys, is at present engaged in a self-imposed soul searching. But this is not a sign of weakness. On the contrary, it is a sign of strength, it is a promise, even a guaranty, of greater successes and of increased usefulness tomorrow. In the attempt to place his science on a firmer foundation, the mathematician may arrive at a better insight, both for his own benefit and for the benefit of others, into the very nature of logic and of logical thinking in general. But he may also learn that the mathematical enterprise in which he is engaged is not independent of the time and the place in which it is being carried out.

SCIENCE ON THE MARCH

BACTERIAL VIRUSES (BACTERIOPHAGES)

WITHIN the past ten years the knowledge that bacterial viruses (bacteriophages) are similar to animal and plant viruses in size, chemical composition, and the necessity of having an intact host cell for multiplication has stimulated much interest in the bacteria-bacteriophage systems.

One of the main reasons why investigators have recently centered their attention on the bacterial viruses has been the realization that these viruses lend themselves to a study of virus formation. The bacterial viruses can be assayed with greater accuracy than most other viruses, and experiments can be carried out quickly. Various analyses can also be carried out with more facility, as the environmental conditions can be readily controlled. Recent experiments indicate that a study of bacteriophages will lead to great advances in the knowledge of the mechanism of virus interference and virus mutation.

Most of the work in this country has been carried out with a nonmotile strain of *Escherichia coli* and seven strains of bacteriophage that can multiply on this bacteria. These viruses have been numbered from T_1 to T_7. The bacteriophages have been classed into groups in respect to serological behavior, physiological characteristics, morphology, and host range. Morphologically, the viruses may show a tailed structure. Such an example is T_1. This virus, under the electron microscope, shows a head of 50 mμ in diameter and a tail 150×15 mμ. T_3, on the other hand, has only a head, 45 mμ in diameter. The average number of virus particles liberated per cell is different for each virus, varying from 120 with T_1 to 300 with T_7. The time before the bacteriophages are liberated from the cell also varies for each virus strain. The virus strains also differ by the type of circular holes, or "plaques," they cause in a continuous layer of bacteria grown on solid medium. These plaques are due to the lysis of the bacterial cell by the virus. Some *E. coli* virus strains form small plaques, some medium plaques, and some large plaques, the plaques varying in diameter from 0.2 mm to 3.0 mm.

The over-all formation of bacterial viruses may be looked upon as occurring in three steps. The first step is the adsorption of the virus to the cell. This reaction stops the multiplication of the cell. The time of adsorption is different with the various strains of virus and may take 2–10 minutes. The physiological condition of the cell influences the time of adsorption, old cells taking a longer time to adsorb the virus than young cells. The type of reaction involved in the fixation of the bacteriophage to the cell is unknown. It had been thought that the process was similar to the type of reaction occurring in the union of antigen and antibody with complementary surface groups on the bacteria and bacteriophage causing the fixation of the virus to the cell. A recent discovery by T. F. Anderson has shown that certain of the virus strains need tryptophan in order to become adsorbed to the host. This finding may mean that an enzymatic reaction is involved in the fixation process. The adsorption of the virus to the bacterial cell is important in another connection: bacterial mutants arising by spontaneous mutation may be resistant to the action of bacteriophage. E. H. Anderson has shown that these resistant bacterial mutants need accessory growth factors, but the relation of these factors to infection by the virus is not clear. These resistant cells do not usually adsorb the viruses, so that an understanding of the process of adsorption might lead to a better understanding of this type of resistance to infection.

The second step is the formation of the virus within the cell (to be described in more detail later).

It should be mentioned here that although a bacterial virus becomes adsorbed to a cell, it may not multiply. T_5, for example, although adsorbed, will not reproduce unless calcium is added to the medium. Calcium is not needed for the multiplication of the host. These experiments may therefore indicate that "invasion" by the virus has to be distinguished from adsorption and that calcium is essential for the former process.

At a particular time, the cell bursts and the bacteriophage particles are liberated. This is the third period. Each virus strain lyses the cell at a time characteristic for this strain. This time at which the cell liberates the virus particles is called the minimum latent period. The yield of bacteriophage particles per cell varies tremendously for a given strain of virus. T_1-infected cells may liberate anywhere from 20 to 1,000 virus particles. According to the work of Delbrück, the lysis of infected cells will occur in 13 minutes irrespective

of the number of bacteriophage particles formed within the cell. The infection of a cell by a bacteriophage seems to start a reaction which causes the cell to lyse independently of the number of virus particles formed in that cell.

Although it seems clear that in the *E. coli* systems studied, the bacterial virus is liberated by the lysis of the host, there have been other bacteriophage systems reported where virus particles are secreted by the cell without lysis of the latter. This condition in which bacteriophage particles are produced without lysis of the cell is known as lysogenesis. It is not clear how the virus is liberated in such lysogenic strains.

When many bacteria are infected by bacteriophage particles, one obtains what is called a one-step growth curve. This consists, according to the work of Delbrück, of three periods: a multiplication of the virus within the cell; a steady increase, for a time, in virus titer as the cells lyse; and a plateau in the concentration of the virus when all the infected bacteria have been lysed. This type of curve has recently been found by the Henles for the growth of influenza virus in eggs.

Little is known concerning the actual formation of bacterial viruses. This is due mainly to the fact that bacteriophage formation can occur only in the intact cell. Once the virus has entered the cell (and electron microscope studies seem to indicate that the bacteriophage penetrates the cell), there is no known way of studying it until the cell is lysed and the virus particles liberated. All attempts to lyse the cell by grinding and enzymatic methods, before the bacteriophage has lysed the cell, have failed to reveal any virus. This fact is all the more peculiar since recent studies with the electron microscope show the cell filled with virus particles before lysis has occurred.

Another obstacle in the path of understanding virus synthesis is the lack of knowledge concerning the normal synthesis of proteins and nucleic acid. Since all bacteriophages studied so far seem to be composed of at least a protein component and a nucleic acid component, it is essential that the normal synthesis of these substances be understood if one is to understand virus synthesis. Up to the present time, however, we do not know how smaller units of nucleic acid and protein, such as dinucleotides or peptides, are formed, much less a complicated "molecule" like a virus. The foregoing facts, and another great problem in virus formation—the question of self-duplication—show how far we are from unraveling the question of virus formation.

Acknowledging how little is known about virus synthesis, we can say that experiments in the past few years have begun to give us a little insight into the mechanism of virus formation. Reports from this laboratory and the University of California reveal that cells which show no demonstrable multiplication after being inhibited by penicillin will still form bacteriophage; therefore, cell multiplication is not a requirement, per se, of virus formation.

Our experiments and those of Cohen indicate that the virus is synthesized from substances in the medium and not from a precursor in the cell. Furthermore, Cohen's finding that the virus seems to contain predominantly extracellular nitrogen and phosphorus would indicate that the synthesis of the virus probably occurs from small-molecular-weight substances. He also has found that in an *E. coli* organism infected with T_2 (Fig. 1), the ribonucleic acid remains constant and appears to be inert. Protein is synthesized from the beginning of the infection, whereas desoxynucleic acid is synthesized 8–10 minutes after infection. The over-all picture one gathers from these results is that, whereas in the normal cell of *E. coli* synthesis of ribonucleic acid, various proteins, and other substances occur, in an infected cell only virus protein and desoxynucleic acid are synthesized. Moreover, these results indicate that, at least in the infected cell, peptide synthesis occurs before nucleic acid synthesis. Furthermore, ribonucleic acid does not seem to be concerned in protein synthesis as pointed out above. The results

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FIG. 1. T_2 BACTERIOPHAGE
SHOWN INFECTING E. COLI B.

of Brachet and Caspersson seemed to show that nucleic acid was intimately concerned with the formation of protein. Cohen's results indicate, on the other hand, that in an infected cell, the formation of peptides may control the formation of desoxynucleic acid. Further work along these lines would be of great interest.

Our experiments indicate that the synthesis by the cell of the energy-rich donor adenosinetriphosphate is essential for bacteriophage formation. This is of interest in view of the fact that no enzyme systems have been found in bacterial viruses. It is probable that one of the reasons why the bacteriophage needs the cell is to supply the energy for its synthesis.

Experiments in this laboratory disclose that there is a continual competition between the bacterial virus and the host for nutrient materials. Such experiments have been worked out by using high concentrations of bacteria and low concentrations of virus as the inoculum in a synthetic medium. In such systems, the virus will multiply for a time but then stop, although the cells will keep on multiplying. This cessation of virus growth occurs because the bacteria have used up substances in the medium essential for virus formation and these substances cannot be synthesized by the host. Using a *Staphylococcus* bacteriophage, it has been possible to show the need of an unidentified substance in acid hydrolysates of proteins for the synthesis of the bacterial virus.

THE study of mutations has led to great advances in the knowledge of bacterial viruses in recent years. Hershey, Delbrück, and Luria have contributed most in this advance. Although such knowledge is likely to change within the next few years, the present investigations are so important as to warrant discussion at this time.

Before discussing the mutation experiments, however, it is necessary to review briefly the effect of multiple infection on bacteriophage formation. It was found by Delbrück that there was no difference in the "burst" size whether a bacteria was infected singly or multiply with the same virus. However, when a bacteria was infected at the same time with unrelated viruses—T_1 and T_2, for example—the bacteria produced only T_2. Even when T_2 was inactivated by ultraviolet light and not able to reproduce itself, it still prevented the reproduction of T_1. These results, whereby one virus excludes the formation of the other virus, have been called the "mutual exclusion effect." A slightly different result was obtained on infecting with T_1 and T_7, which are also unrelated viruses. Here the cells formed either T_1 or T_7. Although in the latter case the infected cell can form only T_1 or T_7, the virus that is not formed is able to depress the yield of bacteriophage from this cell. This is called the "depressor effect."

The mechanism whereby one virus can interfere with the multiplication of another is not known. Delbrück has proposed a "penetration hypothesis." According to this theory, the entrance of a virus into a cell changes the cellular surface in such a manner that the other virus cannot enter. This explanation may hold for the bacteriophage cases studied, but it does not appear to hold for all bacterial viruses. Hershey has recently described a mixed infection involving a wild type strain, T_2, and its *r* mutant. On bursting, the cell liberates both wild type and mutant. Further, mixed infections with related pairs of viruses of T_2, T_4, and T_6 give only partial exclusion. Syverton and Berry have also reported that three different animal viruses—papilloma, myxoma, and B virus—may all infect and multiply in the same cell.

Delbrück has recently published experiments showing that, in a mixed infection, although a virus may not multiply, it may "induce" changes in the virus that does reproduce. Using coli bacteriophages, T_2r+ and T_4r, he found that some cells liberate only T_4r, but that other cells liberate both T_4r and T_4r+. Therefore, while T_2r+ was unable to multiply, it could "induce" T_4r to mutate to T_4r+. In view of the recent finding by Avery and co-workers of an induced change in pneumococci by desoxynucleic acid, it would be of interest to determine whether a nucleic acid from the T_2r-phage would, when added to a system containing bacteria and T_4r, give rise to T_4r+. This result might be able to account for Hershey's finding that a single cell is capable of liberating both the wild type virus and its *r* mutant. As in Delbrück's experiments, only one virus may actually multiply, but the virus not multiplying may "induce" a mutation in the virus that multiplies. Further work is necessary to explain this phenomenon.

Luria has recently published experiments which may help explain some of the mutations observed in the mixed infections. He showed that two bacteriophages, both inactivated by ultraviolet light, will give rise to an active bacteriophage if made to infect a single bacterium. He has proposed that bacteriophages are made up of 30–50 loci and that each of these loci can be transferred from one bacteriophage particle to another inside the bacterium. Luria suggests that bacteriophage formation occurs by independent reproduction of each of these loci in the bacterium. It is not easy to visualize

how such an exchange of loci could occur. As was shown by Schoenheimer some years ago, however, there is a continual changing over of molecules in an organism, with newly formed small molecules re-entering specific portions of large molecules, so that the fact that there appears to be an exchange of "units" between bacterial viruses may not be so unusual a phenomenon as it would at first seem.

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THE PROBLEM OF THE HELICOPTER

NOTHING could be simpler than the basic principle of the helicopter. A rotor, similar to an airplane propeller, but with a much larger diameter relative to the aircraft, and a smaller pitch or blade inclination, rotates slowly with its axis substantially vertical. The rotor impels the air *downward* and therefore by Newton's law produces a thrust *upward*. The upward thrust gives the necessary lift to the aircraft. If by appropriate means the axis is inclined forward, the rotor thrust provides propulsion as well as lift and the helicopter moves forward. Alternatively, we can say that the helicopter rotor combines the functions of an airplane wing and an airplane propeller, and that seems to be a welcome simplification. Besides, the airplane wing only exercises lift when it is in forward motion by dynamic reaction. The helicopter rotor can produce thrust, or lift, when the aircraft is *not* in motion. Therein is the explanation of the wonderful capacities of the helicopter, which can hover motionless above the ground, climb vertically upwards, descend vertically, and travel in any direction following the inclination of the rotor axis. Thus, the helicopter surpasses the airplane in versatility and does almost as well as a mosquito. The helicopter pays heavily for these added capacities, however, and is inferior in speed and pay load to the airplane. Also, it is a more complex mechanism than the airplane, and its designers have to solve many curious problems.

To be efficient in lift the rotor has to handle large masses of air and impart to them a low downward velocity. Then momentum change and thrust are obtained without too great loss in the kinetic energy of the slip stream. That is why the rotor has to have a large diameter and to revolve slowly. But if the rotor is large and revolves slowly, its torque, or turning moment, is correspondingly large. Somehow this torque has to be counteracted; otherwise fuselage and occupants would revolve dizzily in space. This first problem of counteracting the torque is so important that it dictates the whole configuration of the helicopter. One method is to use a tail rotor that revolves about a horizontal axis and provides a lateral thrust. This is the classical solution which Igor Sikorsky adopted and which is apparent in our photograph of the Sikorsky S-51 helicopter. Unfortunately, the power used up by the tail rotor is pure and continuous loss, the tail rotor is clumsy, and it may be dangerous to careless spectators. Yet the single main rotor-cum-tail rotor configuration is the simplest and the one most popular today. Superimposed coaxial rotors revolving in opposite directions, or rotors placed on either side of the fuselage and also revolving in opposite directions, eliminate the torque but introduce difficulties of their own. However, no one can yet say what will be the final configuration.

Another difficulty of the helicopter lies in the fact that in forward flight the advancing blade of the rotor has a greater velocity relative to the air than the blade retreating from the wind. Accordingly, the advancing blade develops more lift and tends to roll the craft over to one side. Juan de la Cierva, the great pioneer of the Autogiro, solved the difficulty, so it is said, while hearing and paying little attention to an opera. Cierva allowed the blades to hinge freely at their roots, so that they flapped upward and removed the excess lift of the advancing blade. The Autogiro which Cierva invented has disappeared; but the merit of his hinged blade is universally realized, and without the hinged blade or its equivalent helicopter flight would be well-nigh impossible.

It is true that the helicopter can rise and hover, but, people ask, what happens when the power fails? That is a good question. The answer is reassuring. Provided the pilot or an automatic control decreases the pitch of the blades quickly, they continue to rotate. When the rotor is in autorotation, the helicopter becomes an autogiro. It can no longer descend vertically with safety, but it can still make a steep glide and land and come to rest after a very short run, in the smallest of fields.

There is one drawback to the helicopter which remains: lack of inherent stability. The airplane in forward flight can be flown hands off the stick and feet off the rudder bar for several minutes. If the airplane has an automatic, gyroscopic pilot, it can conceivably fly without the pilot's guidance for an hour at a time. In the helicopter no automatic pilot is as yet available, and this type of aircraft has not

SIKORSKY S-51 HELICOPTER

LANDING ON THE AIRCRAFT CARRIER U.S.S. FRANKLIN D. ROOSEVELT DURING ATLANTIC FLEET TEST OPERATIONS.

yet developed inherent stability of its own. As a result, the helicopter pilot has to fly his craft the whole time, and that means fatigue. The problem is important and remains to be conquered.

Accidents due to failure of the rotor are very rare; when they occur they are serious, because the centrifugal force unbalance when a blade lets go is enormous. Failure of the rotor is a matter not of strength but of vibration. In an airplane in steady flight the fixed wing always meets the air in precisely the same way at any instant. In the helicopter rotor the blade meets the air at each point of the cycle in a different way; hence, aerodynamic excitation forces occur. Again, because the blade flaps as it revolves, dynamic forces arise, forces that the engineer terms Coriolis forces. At any rate, there are exciting forces, and vibration follows. The reduction of exciting forces and their isolation from the rest of the aircraft constitute a difficult problem. The physicist will appreciate the situation when he is told that the dynamic equations of motion of the helicopter blade contain variable coefficients and cannot be solved. The vibrations of the rotor also feed back into the controls and produce "stick shake." It is difficult to say which offers the most beautiful field of research for the applied mathematician, helicopter stability or helicopter vibration.

How is control attained? By changing the pitch of the blades at certain points of the cycle, periodically. The mechanism is simple in principle, complex in execution, and involves careful mechanical design. Since the rotor revolves slowly while the internal combustion engine revolves rapidly, the rotor must be geared down about 10 to 1, and the transmission system becomes heavy and complicated. Other mechanical problems lie in the transmission of power to the tail rotor, and in the rapid change of pitch of the rotor for autorotation. The cooling of the engine is also a difficult problem because the engine of the helicopter may be required to deliver its full power while the aircraft is stationary. In brief, it can be said that the mechanical part of the helicopter is much more complex

than the mechanical part of the airplane. Moreover, it is impossible to use in the helicopter those standard automotive parts that are available for the motor car. For the same duty, helicopter mechanical units must be much lighter and of more refined construction.

A photograph of the Sikorsky S-51 shows a typical, and perhaps one of the most widely used, helicopters of the day. Modern helicopters are remarkable for the roominess of the cabin and the wide vision that both pilots and passengers enjoy. To be able to take advantage of its hovering and vertical landing capacity, the pilot must have extraordinarily good vision. The photograph indicates the long tail boom, at the end of which the torque rotor is mounted. The landing gear is of the well-known tricycle type with a small nose wheel at the front. A machine such as the S-51, equipped with an engine of approximately 500 h.p., will have a gross weight of 5,100 pounds, top speed at sea level of 103 miles an hour, hovering ceiling of about 5,000 feet, and weight empty of about 3,850 pounds. From these approximate figures it is clear that the helicopter can in no way compare in efficiency, speed, or load-carrying capacity with the airplane. The point is the helicopter should not be so compared. It has spheres of its own in which it reigns supreme.

When the helicopter first became in this country a practical aircraft, thanks to Igor Sikorsky's remarkable development work in 1936–37, there was a burst of enthusiasm as to its possibilities. Predictions were made that helicopters would be the private flying vehicle of the future, and that in a few years private owners would operate hundreds of thousands of helicopters. These optimistic expectations have not been realized, for very good reasons. First of all, although it is very easy to learn how to fly a helicopter, its operation, because of lack of stability, vibration, and some degree of stick shake, is fatiguing. Another reason why we have not had wide adoption of the helicopter for private flying is the very high cost of the helicopter to date. This high cost is due to the fact that few helicopters of any one type have been designed and built, and because of the extra complexities of the mechanism. A two-seater helicopter like that manufactured by Bell Aircraft may have a price of some $25,000; a five-passenger machine with more powerful engines may cost $65,000–$70,000. Such costs are almost prohibitive in private use. Private flying with the use of the helicopter will come certainly when some enterprising manufacturer finds ways of reducing costs to more modest levels.

That does not mean, however, that the helicopter is not already at work. During World War II it found a great many applications in rescue work in the jungles and overseas. The Coast Guard found it an indispensable element of equipment. For military operations of all kinds the helicopter already has remarkable utility. In spite of its cost, it is rendering a variety of services to industry and business, services so useful and so superior to services rendered by other vehicles that cost becomes almost subsidiary. Thus, the helicopter is now being used for crop dusting, for patrol of power lines, for detection and fighting of forest fires, for geophysical and mining exploration, and for many other purposes where its special ability comes into play.

American helicopters lead the world and have

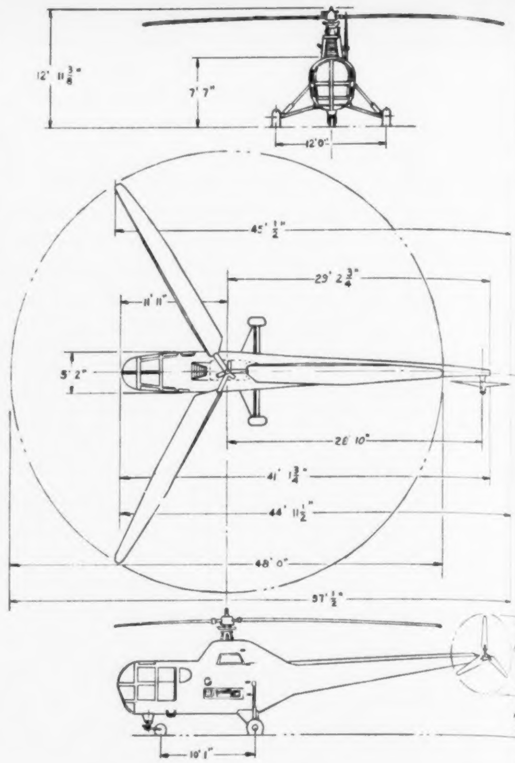
ARMY-SIKORSKY R-5F HELICOPTER

been sold to Great Britain, Sweden, Holland, Argentina, Brazil, and many other countries. Young men have come from the four quarters of the world to learn how to fly the American helicopter. It is also gratifying to think that the helicopter has saved far more lives than it has destroyed.

The Post Office Department, after a series of experiments, has contracted with Los Angeles Airways for a system of carrying mail by helicopter, connecting the center of Los Angeles with many far-flung local post offices. The experiment has proved most satisfactory, and there is no doubt that similar services will be extended to other metropolitan areas such as New York, Chicago, Philadelphia, and probably Boston. Such services would make a tremendous difference in the time of delivery of air mail, and supply the missing link in the air mail service of the United States.

No attempts have yet been made to carry passengers from the airports to the hearts of cities, but there is very little doubt that the ferrying of passengers will soon come. The Piasecki helicopter is already available for the carrying of ten passengers, and estimates of expenditures indicate that ferrying of passengers from the heart of cities to airports is economically feasible. The Civil Aeronautics Board hesitates, on the grounds of safety, to approve the carrying of passengers. The helicopter can land on roof tops or on any odd city lot 100 feet by 100 feet, but there is some hesitancy in putting it to work in thickly populated districts, with skyscrapers inducing unexpected and violent air currents.

In conclusion, it may be said that, although the helicopter is never likely to equal the airplane in long-range transportation, it has already demonstrated its utility in special applications; nor is there doubt that such utility will grow. Also, it seems particularly well suited for private flying. Nothing could be more practical than the helicopter that could land, so to speak, in a man's own back yard. One can be sure that American ingenuity



PRINCIPAL DIMENSIONS OF SIKORSKY S-5

FRONT, TOP, AND SIDE OF HELICOPTER (NAVY DESIGNATI HO3S-AIR FORCE R-5F). OVER-ALL MEASUREMENTS A HEIGHT, 12′ 11⅜″; LENGTH (BLADES EXTENDED), 57′ MAIN ROTOR TIP CIRCLE DIAMETER, 48′; TAIL ROTOR T CIRCLE DIAMETER, 8′ 5″; LANDING GEAR TREAD, 1 FOR EASE IN HANDLING ON SHIPBOARD, MAIN ROTOR BLA MAY BE FOLDED BACK ALONG TAIL CONE TO REDUCE T GREATEST WIDTH TO THAT OF THE LANDING GEAR AND T GREATEST LENGTH TO 44′ 11½″. CABIN WIDTH IS 5′ 2

will not be long in conquering the difficulties th now delay the use of the helicopter in private flyin

ALEXANDER KLEMI

Helicopter Editor
Aero Digest

BRITISH INSTITUTIONS FOR RESEARCH IN SOILS AND CROP PRODUCTION*

WHATEVER kind of farming a man undertakes, he is ultimately dependent on the soil and the crops, and so it was natural that British agricultural education and research should begin with these two very important subjects. There had long been spasmodic efforts to study them, but the modern period began rather more than one hundred years ago when three organizations were started that have since played a great part in ou rural life: the Royal Agricultural Society, th Royal Agricultural College, and the Rothamste Experimental Station. They began quietly and i a small way, in a period not unlike the presen when England was greatly impoverished by year of war at sea and on the Continent, when the were great social and political upheavals, and man people thought the country was going to the dog But under the troubled surface many movemen

* From a speech given over the British Broadcasting Corporation network.

were developing which later emerged and greatly helped the people of those days, and are proving even more useful now. The Royal Agricultural College did great service in training men; the Royal Agricultural Society started a far-reaching research program that has not yet been exhausted; and the Rothamsted Experimental Station has carried out agricultural experiments from its beginning.

The Station began in typically British fashion. Rothamsted is not a village or a town, but an ancient manor which remained during the first six hundred or seven hundred years of its existence quite unknown to history; and it might have remained unknown but for the fact that the young squire, John Bennet Lawes, coming into his inheritance in 1836 and anxious to increase the output from his land, was asked by a neighbor why ground bones were so good as manure on some soils and so poor on others. Lawes knew a little chemistry: he realized that bones contained a phosphate which was insoluble in water but would become soluble on treatment with sulphuric acid; he thought it should then be effective on his land. He tried the experiment, and it was so. More important still, geologists were at that time discovering large deposits of mineral phosphate for which there was no obvious use. Lawes thought that this would serve instead of bones, and experiment showed that he was right. He took out a patent and set up a factory for making this soluble phosphate on a large scale. Thus he was able to supply farmers with a new fertilizer—superphosphate—now famous throughout the world. That was the beginning of the commercial fertilizer industry. Lawes made a fortune out of it. But he did not stop there; in association with Joseph Henry Gilbert he carried out from 1843 onward a series of scientific experiments on soils and crop production on his farm at Rothamsted that are now known wherever agricultural science is studied.

It was relatively easy in those days for these two very intelligent and industrious men to make scientific discoveries. They were pioneers, and the nuggets were, so to speak, lying on the surface. They had all the sturdy independence of the Victorians. Lawes paid the whole cost of the work and would have scorned any idea of Government help. The work was practically all chemical, for at the outset agricultural science was just a branch of chemistry. In these days, it is of course very much wider and more complex: biochemistry, physics, mathematics, pedology, and some half a dozen branches of biology all now come into agricultural science. The modern Rothamsted research station that has grown up during the past thirty years has well-staffed and well-equipped departments dealing with all these subjects. Nevertheless, the Chemical Department, under Dr. Crowther, is still one of the most important. The director is now Dr. Ogg.

Rothamsted remains primarily a research institution, but, like most other British research institutes, it can take in a limited number of students who have already graduated and who therefore have a good knowledge of their science. Men and women come from all over the civilized world to work there, many bringing new ideas—and new ideas are the lifeblood of a research institution. It is a postgraduate school of the London University, and students having a Bachelor's or Master's degree acceptable to the Senate may become candidates for the Ph.D. or D.Sc. degrees without further examination. These degrees rank very high, especially the D.Sc., and their proud possessors are to be found in important posts in many countries.

There is another, smaller Soil Institute at Oxford University under the direction of Mr. Morison. He has specialized in the soils of the Southern Sudan and has collected material of great value for students of tropical soils. Graduates of recognized universities can obtain a degree after two or three years' study, according to their qualifications.

Scotland also has a soil research institute, the Macaulay Institute at Aberdeen. Unlike Rothamsted, it has local duties and affiliations, and is responsible for the Soil Survey for Scotland. A student who wishes to gain experience in that type of work could usefully spend some time there. The Institute possesses equipment for studies in soil mineralogy, an important new branch of soil science.

Wales has an important center of soil studies at Bangor, under Professor G. W. Robinson, who was until recently in charge of the Soil Survey of England and Wales, and who also can receive postgraduate students.

An important group of agricultural research institutes has developed at the Cambridge University School of Agriculture started by T. B. Wood, one of the greatest of the pioneers. With him was associated Sir Rowland Biffen, whose inspiration came from two great figures in English biological science, Marshall Ward and William Bateson. Marshall Ward was the founder of modern plant pathology in Britain, and Bateson of modern genetics. Biffen, a student of both, combined their subjects and bred wheats to resist rust, giving

them also other desirable qualities. Thus, Cambridge became a great center for the breeding of wheats for English conditions, and the Cambridge wheats are widely and successfully used. Hunter bred barleys at Cambridge. The work is now carried on under Professor Engledow, and a well-known postgraduate school has developed where prospective plant breeders from overseas can profitably study.

Fodder crops, such as oats, grasses, and clover, however, are not dealt with at Cambridge but at the University College of Wales, at Aberystwyth, where the climatic conditions are more favorable for the breeding and selection of that group of plants. This work was started by Sir George Stapledon and has attracted widespread attention in many parts of the world, particularly since one of the most urgent needs of today is the improvement of pastures and grazing lands. Sir George has now retired, but the work continues under the competent direction of Professor T. Jenkin. Apart from the valuable help they will receive at the Plant Breeding Station, postgraduate students will find much to interest them in other departments of the College and its beautiful surroundings.

A grassland improvement station has also been set up at Drayton, near Stratford-on-Avon. Although it is not equipped for sustained postgraduate studies, it can furnish material for a student centered at a University. Useful work on the production of varieties for northern conditions has been done at the Scottish plant-breeding station at Corstorphine, near Edinburgh. Potato breeding is undertaken there, but it must be admitted that the greatest success with this crop has been attained by the commercial breeders, artists in their line, who have constantly before them the ideal of the perfect potato, and spend long years of effort in trying to attain it.

The other great branch of agricultural science inspired by Cambridge—plant pathology, the child of Marshall Wood—has spread rather widely, but is still largely being developed by Cambridge men. Dr. Kenneth Smith at Cambridge deals especially with virus diseases of plants, and the same subject is being investigated by a vigorous group of workers under F. C. Bawden at Rothamsted—without overlapping the Cambridge work. These diseases are spreading in all parts of the world and are doing tremendous harm, causing great losses to important crops. Much has been learned about the nature and properties of the different viruses and the way they are transmitted—a fascinating story. When you realize that some of these diseases are transmitted by the aphis, the little fly of the same family as the notorious green fly of roses; that the aphis sucks up juice from a diseased plant and so imbibes some of the virus, then carries it to a healthy plant and infects that—when you think what minute quantities are involved, you will marvel at the skill of the experimenters who discovered the details of the process, dissecting the aphis to find out how the transmission is brought about, what changes the virus undergoes during transit, what the virus does when it gets into the plant. It is a good subject for a keen young biologist.

One might not at first think of higher mathematics as having anything to do with agricultural science, and indeed it is only in the past twenty-five years that it has come in. The beginning was made at Rothamsted by R. A. Fisher, who is now at Cambridge, dealing with genetics from this standpoint. The work was ably carried on by F. Yates, and a strong department under him provides ample scope for young mathematicians with a leaning toward biological science.

A young but promising department has been set up at Oxford dealing with the design of agricultural and general biological experiments. It is not everyone that combines the necessary mathematical ability with a sound biological sense, and fewer still have as yet equipped themselves for the posts of statistical advisers to the experiment stations. But the need is there, and the demand must grow.

Overseas students with the necessary scientific training will find no special difficulties in entering a British research station for study, provided always there is room. The stations are not, however, equipped for training, and they provide no systematic courses, though they are nearly all closely associated with universities, where course teaching is available—but always on the same condition: provided there is room. For our colleges and universities in Britain are fuller than ever: there is a great demand among our young people for more and better education. In all material things life here is austere, though no overseas student need fear that he will go hungry. The intellectual and cultural life of the nation still goes on as before. Science and the arts, music, the drama, painting, still flourish vigorously, and the student will find ample provision for all his intellectual needs.

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DESCRIPTION OF ROCK COLORS

MINERALS display a variety of colors. Indeed, minerals and mineral products compose many of the pigments used in paint manufacture. In descriptive mineralogy one of the distinctive properties of each mineral is color; another is streak, the color of the powdered mineral.

Rock colors are less vivid yet diversified. Weathering changes the composition and the color. Books about sedimentary rocks cite color as evidence of the origin and past history of the rock and its constituent minerals. It is controversial whether red beds signify aridity or tropical weathering. "The environmental conditions determining the colors of sediments are not fully understood, but are of great importance."[1]

One may be surprised, therefore, to discover that the color names used are referred to no standard. The specific gravity, hardness, crystal form, index of refraction, and composition of a mineral are all determinable. Not so, the color. The mineral determines the color rather than the color the mineral. "Ruby-red" is the color of a red ruby.

The description of rock colors is in worse state. With notable exceptions, most geologists name rock colors offhand. Their choice is affected by the immediate surroundings and by the weather, is inconsistent from day to day, and is very individual, having little regard for the usage of other geologists. I asked thirty-four petroleum geologists, who describe rock cuttings daily, to specify the colors of 14 different rock specimens. One specimen received 28 different specifications.

About 150 years ago Werner, father of mineralogy, worked out an arrangement of colors and color names, illustrated by a set of mineral specimens. He may be regarded as having founded the scientific standardization of color names for all purposes. "The following eight colors were selected by Werner as fundamental, to facilitate the employment of this character in the description of minerals: *white, gray, black, blue, green, yellow, red,* and *brown.*"[2] Under the guidance of Professor Robert Jameson, another mineralogist, Werner's system was published in book form in 1814.

Robert Ridgway, an ornithologist, published his book on *Color Standards and Color Nomenclature* in 1912. It was referred to as a standard for both rocks and maps by the United States Geological Survey. In the descriptions of rocks it yields such terms as "cinnamon-rufus" and "deep Corinthian."

In 1928 Goldman and Merwin abridged Ridgway's 1,115 colors to a set of 114 designed for use by field geologists. The chart[3] did not attain general use, partly because it employed rather forbidding symbols and gave no color names.

From 1915 on the Textile Color Card Association of the United States issued its "Standard Color Cards." These have been used as standards by some geologists.

The Maerz and Paul *Dictionary of Color*, published in 1930, is an excellent reference work but not a field manual. It illustrates 7,056 different colors and, undertaking by means of these to define all usable color names in the English language, it offers a wide choice. Geologists using it have startled their colleagues by describing the color of a limestone as "French nude."

Since Newton and Lambert, it has been recognized that a particular color is the summation of three effects. One of these, called lightness, or value, is illustrated by the range of the neutral colors from black through the grays to white. Value can be measured directly by physical means as the reflectance.

Another of these effects is hue. Newton arranged all the gradational hues of the spectrum from red through yellow, green, blue, to violet on a circular arc. He completed the circle by means of the purples, which are mixtures of blues and reds.

The third effect is chroma, which is the inverse of grayness. Complementary hues may be placed at opposite poles of a diameter of the hue circle. Complementary hues mixed in the right proportion make a neutral gray, which can also be approached by adding gray to either one of the hues and thus reducing its chroma. The chroma of any hue may be said to range from vivid through bright and moderate and weak (or grayish) down to the very absence of the hue represented by a neutral gray as the limit of the sequence.

Since each color is a function of the three variables, value, hue, and chroma, it is possible to represent the whole array of known colors by a three-dimensional diagram or color solid. For practical geological purposes consider the color solid to be a sphere with a diameter representing the neutral axis from black at one end to white at the other. A radius perpendicular to the middle point of this axis revolves through the whole hue circle. Each radius, then, is a representation of the chroma of a particular hue from gray at the axis to vivid at the surface of the sphere. Given a particular hue and chroma represented by the end point of some radius vector, there is still a range of lightness from darker to lighter represented by a line through the end point parallel to the neutral axis.

The Munsell color notation devised by A. H. Munsell (1858–1918) divides the value into 10 major units from 0 (black) to 10 (white). His 10 hue units are *R, YR, Y, GY, G, BG, B, PB, P,* and *RP,* the letters referring to red, yellow, green, blue, and purple. Each hue unit has 10 subdivisions of equal arc on the hue circle; for example, 1*YR*, 2 *YR*, . . ., 10 *YR*. Chroma in the color sphere ranges from 1 to 10 (and also above 10, but few rock chromas exceed 6).

Munsell chose these units to represent equal psychological steps in color perception. Their real value in scientific measurement is that each color is carefully reproduced by the Munsell Color Company and is physically specified by means of photometric measurements. A Munsell designation such as 5*YR* 6/2 (where 6 is the value and 2 the chroma) is physically fixed and internationally communicable without recourse to native languages.

Soil scientists and others who study drugs, chemicals, and agricultural products have already adopted the Munsell system as a basis for color description. Color experts are determining the Munsell designations of the 7,056 colors in the Maerz and Paul *Dictionary*. When this work is completed it will provide an immediate correlation between standards.

In the past two years a Rock-Color Chart Committee, composed of representatives of leading geological organizations, has determined the range of rock colors by comparing a wide selection of specimens with Munsell standards. Rock colors are concentrated in the *YR*s and the *Y*s. The green, blue, and purple rocks are relatively scarce and very grayish; that is, most of them have chromas less than /2.

Following the lead of the Inter-Society Color Council and the National Bureau of Standards,[4] the Rock-Color Committee has tried to select a system of rock-color names sufficiently standardized to be acceptable to color scientists, sufficiently useful to be satisfactory to field geologists, and sufficiently commonplace to be understood, at least in a general way, by anyone concerned with rocks. A new color chart for field geologists illustrates these color names by means of chips supplied by the Munsell Company. The first copies will soon be available.

Each color chip on the chart is marked by a name and a Munsell designation. A color name, such as "pale red," occupies a portion of space in the color solid, whereas a Munsell designation is represented by a point. Munsell designations are therefore more accurately descriptive than names, and numerical interpolation is easy between colors described by numbers. The Munsell system gives a definitive answer to the old question, What happens to the color when a rock is wet? The hue and chroma remain unchanged, but the value is diminished. In other words, the color is darkened only.

Although the Committee has established the range of rock colors and designed a field chart, the mechanical problems of arranging Munsell standards for microscopic and other laboratory use remain to be solved.

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BOOK REVIEWS

UNDERGROUND WEALTH

Mineral Resources in the United States. Bureau of Mines and Geological Survey. viii + 212 pp. Illus. $5.00. Public Affairs Press. Washington.

NEARLY sixty members of the staffs of the Bureau of Mines and the Geological Survey have pooled their knowledge to compile and synthesize the information contained in this volume. Like any compilation, it is not designed to be read at a sitting, but it should be required reading for those to whom it is addressed. According to Secretary Krug, these include "legislators, Government officials, producers and consumers of minerals, and others concerned with problems of mineral supply." Obviously, it is little more than a pious hope that many legislators and government officials will absorb the basic facts which the book contains, even if copies come into their possession, for it takes a different psychological approach than is embodied in a compendium to reach the average congressman or government administrator.

An introductory section sets forth the objectives, methods, and results of the survey, providing an honest appraisal of its limitations. To anyone familiar with the mining industry, the imperfections of knowledge regarding any ore body being developed are well known. There are cases in which engineering and geological skills have been applied to the delineation of the deposits with such painstaking care that reliable estimates of tonnages are available. As the authors make clear, these are the exceptions; hence, the estimates of reserves for any metal or mineral can be no more accurate than the data from the individual deposits which, in sum, comprise the nation's reserve.

A second section discusses trends in the several phases of the mineral industry, including exploration. In the limited space devoted to this subject it is obviously impossible to do justice to its many ramifications, but the private operator is bound to feel that something less than justice has been done to individual and corporate initiative in developing technological processes and devices which have expanded reserves and increased recoveries, or created new uses for old and new mineral products. Even so, it is a useful summary with a cautiously prophetic viewpoint.

Approximately three quarters of the book are devoted to brief but careful surveys of the known and prospective reserves of thirty-nine individual metals, minerals, and fuels which are of critical importance in our industrial economy. Quite literally, the subject is covered from A (*Arsenic*) to Z (*Zinc*), drawing upon the vast fund of private and public information placed at the government's disposal during the war. Such a survey could have been prepared only as a consequence of a national emergency that prompted or forced individuals and companies to divulge information privately acquired and privately held. Its publication is timely, for it provides an inventory of resources at a moment when we need to take stock of our raw materials for the immediate future. The composite picture is more encouraging than our professional pessimists would have us believe, yet, in a list of thirty-nine minerals, this nation falls short of self-sufficiency in twenty-seven, for ten of which we produce less than 10 percent of our requirements.

It is probable that the book will be criticized for its self-imposed limitations, and for the scrupulous preservation of anonymity for the industrial and mining organizations that comprise the mineral industry. Criticism of this kind is gratuitous, however, for this is not a treatise on the mining industry but on our national mineral reserves. The authors have achieved their goal with commendable success, though one may reasonably lament the mediocre proofreading and the poor job of printing from which the volume suffers. Scientists will perform a patriotic duty if they will make a sincere effort to see that the book comes to the attention of the legislators and officials for whom it was written. It is reasonably certain that the producers and consumers of minerals will not miss it.

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FACTORS IN BIOLOGICAL BALANCE

Birds of Prey of Northeastern North America. Leon August Hausman. xxv + 164 pp. Illus. $3.75. Rutgers Univ. Press. New Brunswick, N. J.

IF FOR no other reason than the fact that it forges another link in the desperately needed chain of favorable publicity for our birds of prey, this book would be useful, and it should go a long way toward breaking down the almost impregnable wall of blind ignorance and prejudice against these truly wonderful and necessary creatures. Dr. Hausman has cleverly woven a strong thread of constructive conservation through every page of this interesting work. He has done this in a very clear-cut manner and has carefully avoided classifying himself as a long-nosed "dickey-birder," an attitude that has alienated many of the very persons that well-meaning conservationists try to win.

Birds of Prey, although conveying the impression that it is a technical work, definitely is not. It deals in generalities and should have appeal to the student not too far advanced in bird study. The competent observer of raptores, however, will be disappointed not to find the exacting data he would expect in a book of this type.

If, as I believe, this book is intended, not for students

but for ordinary readers, such readers will find themselves much more familiar with the personalities and the intricate relationship of our birds with the rest of the living world than before.

There is a line illustration covering each species, and these are quite good considering the limitations of simple line technique. Naturally, full-color paintings lend warmth and are of much greater value for purposes of identification.

T. H. CUNNINGHAM

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EVERYBODY'S AGRICULTURE

The Business of Farming. Herrell DeGraff and Ladd Haystead. xviii + 244 pp. Illus. $3.00. Univ. of Oklahoma Press. Norman.

THE title of this book implies that a reader could expect to find most of the problems discussed that affect profits and losses in farming. This book does admirably on production phases. It briefs one almost equally as well on organization and reorganization of the income enterprises on a specific farm or area of land. It does not emphasize nearly so well the problems of credit, value of lands, first costs, hazards in owning property, debt repayment, and loss of property owing, not to inefficient management, but to error at time of purchase.

One finds more than the usual proportion devoted to productive phases of the farm and less to farm management or ranch organization. Soil- and water-management problems hold the prima donna position whether it be on a 10-acre tract, 160-acre standard, or a 3-section wheat farm in the Plains.

The authors should not lose sight, however, of the almost equally important phase—economic disasters caused by purchase of either good or poor land at too high a value at the beginning of one's "business of farming."

In American agriculture nowadays a farmer, to be successful, must produce yields above average for his community. To do this he requires technical information on the variety of seed to plant, kind of fertilizer, method of applying fertilizers, control of plant diseases by new kinds of chemical compounds, and newest labor-saving equipment, as well as the cheapest and the easiest way of preserving and storing the harvested product. Recognizing this fact, the authors have mentioned in a general way some of the new techniques in production such as DDT, soil fumigants, hormone feeding, weed killers, vitamin retention, etc.

A young, nontechnically trained reader or a businessman putting his funds in land for investment might think that current know-how in production is more important than future know-how or the problems due to price changes. All except the young and inexperienced know these techniques will be obsolete a few years from now.

The Index could have been two- or threefold longer in order to give more topical descriptions. The inquiring young businessman without a formal training in agronomy or animal science, for example, could after consulting the Index, then write for any of the 134 references listed or see his county agent. The county-agent approach is more effective than references for new techniques in production. References are more effective with economic problems. Commercial money or farm-management services are more applicable to these problems than general information from rural economic departments of land-grant colleges.

For some types of readers—perhaps the busy man financially successful in some other business or the nonacademic, white-collar, salaried man—references to technical information will not be of interest. The college graduate, fresh out of college three to five years, itching to leave his job and get into the beef cattle finishing business, dairying, or raising potatoes, celery, or cabbage, will probably obtain and read all the references on his problem.

The second problem, that of organization, is not as well treated by the authors as production efficiency. Naturally, only general terms and policies can be mentioned. Full credit should be given the authors for attempting to discourage anyone trying to get rich quick in agriculture. In several places favorable reference is made to major income being supplemented by minor-enterprise income, even at an accounting loss. In this day of high-cost specialization in production minor enterprises often take justifiable profit from major enterprises. Today, efficiency goes hand in hand with specialization, not with several enterprises on the same farm. A credit structure to withstand for two or three production periods low income caused by low yields or low prices is the suggested farm organization of some economists. This permits efficiency of production in the major enterprise. The authors might well discuss this in more detail in a revised edition two or three years hence. Labor and equipment management might perhaps be treated as one. They are well analyzed. If any constructive criticism is offered at all on this phase, it is that the approach to these topics is too academic. Labor problems are continually shifting with equipment problems. Obsolescence costs in both machinery and buildings are likely to be many times greater than the reader will expect to experience during the next ten to twenty years if he sets his credit structure as indicated in relation to other costs for the farm.

The treatment of the third and last phase of business in farming—mainly how to keep sales above expenses—is not too explanatory. The topic is treated more or less as if the operator would have a static or fixed level of prices and costs with which to pay interest, taxes, and other costs. Perhaps that is being too critical. On page 176 we find: " . . . . an individual farmer can do little or nothing about price on a bulk market." The reader who is worrying about purchasing at 1947 levels or waiting until later can find little comfort in the book except that if he buys good land, not too high

and applies good management he will average out over twenty to thirty years. There is little to tell him how to tide over that one or two years out of the thirty that may wipe him out entirely. It takes only one such decline in a farmer's lifetime to destroy his twenty-year accumulation of credit.

Price declines and area crop disasters don't disrupt the farm owner for his whole lifetime. It is the mismanagement of credit that permits these two to cause a shift in the farmer's occupation. Some of the most efficient farmers on the best land on the fringes of the corn belt in the early thirties were forced off the land with only the family and the car. Many of them could write a chapter on how to be successful if credit is supervised. Their first item would be not soil management but credit safety. A poor, run-down, badly eroded, once-cultivated farm purchased at less than its potential productive value has made many a mediocre manager appear successful on the books of the farm-management association. In the same associations, the most efficient managers (some ex-county agents), who purchased the best land above its potential productivity value, were not so successful and returned sooner or later to some other occupation.

Good and poor management and good and poor farms make up the four general categories, all equal in opportunity for success if the purchase price is in line with potential productivity value. The amateur reader may get the impression that only good land should be purchased. We often hear the slogan "You can't pay too much for good land." The truth is, it has been done for all types of land.

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OURSELS AS OTHERS SEE US

The American People: A Study in National Character. Geoffrey Gorer. 246 pp. $3.00. Norton. New York.

MOST books on the American people by visitors from other countries have been written by travelers, journalists, and writers and have been admittedly impressionistic. As such they have generally been refreshing and useful. What distinguishes this book, written by an Englishman, is that it is presented to the reader as an anthropological and psychoanalytical study and thereby lays claim to consideration as a scientific treatise. It appears to this reviewer, however, that most of Gorer's generalizations lack documentation and frequently tell but half the truth. Except for a few references to other societies, the fact that the author is an anthropologist has had no apparent effect on his methodology. Much of the material of the book is based upon discussions with university colleagues and their wives (he lists the names of about fifty such informants), who are hardly representative of a cross section of the American people. As a result, most of the author's observations, at best, apply to the middle class. Furthermore, Gorer's glib explanations of American behavior in terms of psychoanalytic concepts applied en masse (the Freudian analysis was developed as an instrument for the treatment of individuals) leave much to be desired and detract from many of his otherwise keen and stimulating observations.

The study of national character, at best a difficult task and one that lends itself more readily to literary than to scientific treatment, is especially difficult in the case of the American people, because of the heterogeneity of the population and the marked regional, rural-urban, and class differences. Mr. Gorer avoids this complexity by limiting his study to a restricted area of the country and to certain groups that he quite arbitrarily identifies with "true Americanism." Thus, he rules out the Southern states, rural New England, and California and ". . . the more obvious minorities, whether ethnic, religious or social . . ." (p. 15). By eliminating the "obvious minorities" and other important Americans who constitute more than a third of the nation's population, Mr. Gorer is no longer studying "The American People." A more accurate title for the book might well have been "Less Than Two Thirds of the American People." In line with this approach, Mr. Gorer distinguishes between "complete" and "incomplete" Americans and lists the Negroes, Jews, Mexicans, American Indians, Asiatics, and foreign-born as among those ". . . whose Americanism is for one reason or another considered incomplete . . ." (p. 211). The author does not raise the question "By what groups are these minorities considered 'incomplete Americans'?" Mr. Gorer, perhaps inadvertently, has adopted the criteria of "Americanism" of some of our most dubious flag-wavers and has mistakenly identified their propaganda with the thought of most Americans. This is further evidenced in his statement that "Intolerance, racial discrimination, terrorism, are perfectly compatible with Americanism; . ." (p. 196).

The psychoanalytic tone of the book is set in the first chapter, entitled Europe and the Rejected Father. For the author the key to the understanding of the American character is the rejection of the immigrant father by his children, a process which he assumes has been going on since the arrival of the Mayflower. He derives many of the fundamental themes in American life, such as the idea of equality before the law, equal opportunity, and the dislike of authority and consequently of government, to the early rejection of the immigrant father who was the symbol of authority and all that was foreign. In similar fashion he explains the high status of American women and the active social role of the middle-aged women. This type of oversimplified psychological approach is also used in his treatment of complicated political phenomena. Thus, he explains ". . . the pathological hatred felt for the late President Roosevelt and his family by so many of the most respected and respectable Americans . . ." (p. 35) in terms of the rejection of the father and hence of authority. But then he writes that

". . . Roosevelt was loved by the less assimilated and less assured groups . . ." (p. 35); i.e., by Mr. Gorer's "incomplete Americans" who apparently had not yet learned to hate authority despite the fact that according to the author's scheme they should have been actively rejecting their fathers. Mr. Gorer's confusion arises from his failure to realize that many of the Roosevelt-haters did not fear authority as such, but feared that Roosevelt would not use his authority in behalf of their vested interests.

Another example of Gorer's psychological explanation of American behavior is his suggestion that because American babies are bottle-fed by the clock they suffer hunger and frustration which in adulthood causes them to fetishistically admire women's breasts and drives them to drink—milk! (pp. 77–78). For all Gorer's sophistication, he seems to this reviewer to be a babe in the American woods.

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CATALYSIS, ESPECIALLY IN RELATION TO LIFE

Life, Its Nature and Origin. Jerome Alexander. vii + 291 pp. Illus. $5.00. Reinhold. New York.

SCIENTIFIC books range between two extremes. There are some that quickly lead the reader into the very heart of their subject, by skillfully displaying its most important features, in logical sequence. Their success is in being orderly. At the other extreme are books that carry such a burden of detail that they are of profit only to those who are already familiar with the subject. To such readers a book of this class may be especially rich in suggestions, from the very fact that the author industriously collected ten thousand more or less pertinent facts, then could not bear to discard a single one of them in order to clarify his main story.

This book is of the second type. Its title should have been "Catalysis, Especially in Relation to Life." The author's thesis may be summarized by brief quotations, beginning near the middle of the book:

> Life is fundamentally a product of catalytic laws acting in colloidal systems of matter throughout long periods of geologic time [quoted from Leonard Thompson Troland].
>
> The simplest living units of which we have indisputable evidence are the genes.
>
> It is possible that in some cases the gene itself is an aggregate of simpler units. . . . The smaller and simpler the living unit considered the greater the probability that the atomic and molecular units composing it might have come together by mere chance.
>
> The original living catalyst was able to initiate its own duplication.
>
> [To explain the development of complex organisms from simple ones, and the possibility of organic evolution]: Biocatalysts are subject to modification which involves the fixation, at an active catalyst area of a gene or other catalyst unit, of some particle which changes the nature and/or rate of the catalytic change occurring there.

In brief, life is viewed *as the result of*, and as actually *by means of*, self-duplicating but alterable catalytic surfaces.

The final chapter is a philosophic glance at the creature whom, in our more optimistic moments, we hail as evolution's greatest masterpiece. Robert Louis Stevenson sixty years ago described this masterpiece more dramatically:

> What a monstrous specter is this man, the disease of agglutinated dust, lifting alternate feet or lying drugged with slumber; killing, feeding, growing, bringing forth small copies of himself; grown upon with hair like grass, fitted with eyes that move and glitter in his face; a thing to set children screaming; . . .

It seems a pity that the author's views, so often reasonable—or at least thought-provoking—are obscured by needless details, or details presented in the wrong places. How life first appeared in what was then a sterile culture medium, the entire earth; how life spread and developed; how catalysis has shaped its destiny—that would have made a dramatic story. It would have been easy to leave the reader with the impression that God, who is worshiped by astronomers as a great mathematician, must really be a master biochemist, putting catalysis to a multitude of ingenious uses, in every living cell of all the vast creation.

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AFRICAN PRIMITIVES

Tribes of the Liberian Hinterland. George Schwab. xix + 526 pp. Illus. $7.50, paper; $10.00, cloth. Peabody Museum. Cambridge, Mass.

THIS is the report of the Peabody Museum Expedition to Liberia, conducted by Mr. and Mrs. George Schwab in 1928. It covers two trips to Liberia's inner border, during which ninety-two days were spent in the interior: first, from Monrovia on both sides of the St. Paul River; and, second, from Cape Palmas along the Cavally River in the southeast. Nine of Liberia's twenty-three tribes were studied, namely, the Gbunde, Loma, Mano, Ge (Gbe), Gio (Ngere), Tie, Sapa (Sapo), Grebo, and the "Half-Grebo." Two maps of tribal distribution are included, one after that of Dr. Struck, and a larger one (1:534,000) prepared by Dr. G. W. Harley, whose contribution in "editing, revising, and supplementing of the data of the Schwab manuscript makes him in effect virtually a collaborator if not a co-author."

The basic similarities between these peoples have made possible a composite topical treatment of their cultures, rather than separate discussions of the individual tribes. Variations are mentioned where they have been noted, and care has been taken to indicate to which tribes specific statements pertain. The pe

ples of this area are basically agricultural and subsist mainly on rice, supplemented by domestic animals. They also do some hunting, trapping, and fishing. Villages, varying in size from about ten to three thousand individuals, are inhabited by several exogamous, patrilineal lineages, which the author calls "families." Neighboring villages whose lineages claim descent from a common ancestor form the "clan," which was the largest political entity before "paramount chiefs" were established by the Liberian government. Even today, chiefs with authority over an entire tribe are lacking.

One may question the author's terminology at several points, as in using "Palaver House" for council house, "devils," "country devils," and "Bush devils" for officials of the Poro and Sande cults, and "Big Devil" for their principal leader. To be sure, these terms are employed by Africans themselves in speaking the pidgin English current in Liberia. Yet their adoption in place of standard English phrases and their use without quotation marks imply a certain condescension toward African institutions that is out of place in a scientific report.

In its content, this monograph resembles the earlier ethnographies more than the detailed modern studies of African peoples; yet one is impressed by the interesting side lights and details which the author was able to record during his brief field work. The publication of this body of useful information, regarding an area in which the United States has long shown a special interest, fills a large gap in West African ethnography.

The reviewer cannot conclude without quoting the dedication: "To the memory of Harvey S. Firestone, to whom the gratitude of the Liberian people stands as an enduring monument in the Hinterland."

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BIRDS OF THE ANTIPODES

Australian Bird Life. Charles Barrett. 239 pp. Illus. $3.50. Oxford Univ. Press. New York.

FIRST printed in 1945, then reprinted in 1947, Barrett's *Australian Bird Life* is designed as a handy volume, pleasant to read, yet scientific and containing an adequate account of Australian birds. Unquestionably interesting and scientific, since it is based on the author's wide and accurate knowledge of Australian birds and upon the published works of other authors, the volume falls considerably short of being adequate except, perhaps, for those already thoroughly familiar with Australian birds. With only *Australian Bird Life* in hand, identification of more than a very few species would be very difficult.

Barrett treats the birds of Australia by broad groups such as jungle-dwellers, water-lovers, the big land birds, birds of the sea, etc. Each chapter is a narrative description of a single group, with emphasis on the most unusual or outstanding species. Scientific names are included for all species mentioned, in addition to the vernacular, and often numerous popular names. Much factual information based on interesting and authoritative observations of habits, nests, songs, displays, etc., is recorded in each section. The volume is well illustrated with half tones and color plates; one of the latter occupies two pages and illustrates the eggs of thirty-two different species.

The most interesting section is entitled Australia's Wonder Birds. In this chapter Barrett vividly describes the appearance of the lyre and the bowerbirds, as well as their habits and their remarkable ability to build dancing mounds and playhalls and to decorate the latter. The "painting" habit of the satin bird is most remarkable.

The distribution of most species is described, and ranges for many are included, but because the volume does not carry a map of Australia this information is not as useful as it could be. *Australian Bird Life* is a companion volume to the author's *Australian Wild Flower Book.*

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SOCIAL PATHOLOGY

Psychosocial Medicine. James L. Halliday. 278 pp. $3.50. Norton. New York.

THE author of this book is a prominent British psychiatrist. He has undertaken to study the psychological needs of human groups. If these fundamentally social needs are not satisfied, the result is a "sick society." The medical approach to the study of the sick society is called "psychosocial medicine" by the writer. Basically, the book is an application of the concepts of psychosomatic medicine to the illnesses of communities and social groups.

Part One deals with Medical Logic, stating that illness must be regarded as a reaction to material as well as psychological and environmental factors; thus "mechanismic" (mechanistic) and "biological" viewpoints are complementary in considering the cause and prevention of disease.

Part Two describes the concept and the incidence of psychosomatic affections. The author defines them as "bodily disorders whose nature can be appreciated only when emotional disturbances in addition to physical ones are investigated." At this point the psychosomatic aspects of various chronic diseases and their connection with personality types are aptly delineated.

Part Three treats the problem of the sick society, describing the environmental changes in the world of the child and the adult during the past hundred years with their devasting influence on the emotional security of modern man. As a result we find a declining birth rate, decreasing psychological health, and a deterioration of moral and social values, all signs of a threatening disintegration of our Western civiliza-

tion. In order to survive we shall have to rediscover in our lives a sense of social purpose and spiritual values—notwithstanding the fact that we have made grandiose technical advancements and have improved our physical health conditions to an undreamed-of degree. The social health of Britain and the United States is the subject of special chapters.

Altogether, we are indebted to the author's far-sightedness in dealing with some of the most pressing problems of our time.

PAUL WENGER

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SCIENCE IS AMAZING

A Treasury of Science Fiction. Groff Conklin, Ed. ix + 577 pp. $3.00. Crown Pub. New York.

RECENTLY there has been a move to broaden the audience for that type of fantastic story called science fiction by its followers by republishing in book form examples of the genre that first appeared in pulp magazines. The present book is the newest addition to this field. A good idea of the drive for respectability in this field of writing may be had by recalling the evolution of the display title for *Astounding Science-Fiction,* the magazine from which substantially all the recent stories used for reprint have been derived. (In the present case all but four out of thirty stories are from this source.) Back in the thirties this magazine was known as *Astounding Stories.* Later it changed its name to *Astounding Science-Fiction,* giving equal prominence to both parts of the name. At present it retains the latter name, but the *Astounding* has been sharply reduced in size on the cover, and the full emphasis is placed on *Science-Fiction.*

In some circles, this type of fiction has been considered as a possible competitor for the detective story as light reading. Though this seems hardly probable in the near future, it does provide a fairly accurate idea of the quality of these stories. The subjects are, indeed, often much more interesting, but the style and presentation is usually inferior to that of good detective stories. It does not appear, moreover, that the emergence of these stories from the pulps promises any immediate improvement in quality. The present collection, for example, contains a story written for the *Saturday Evening Post* by Robert Heinlein that is decidedly below the standard of the stories he has written for *Astounding Science-Fiction.*

What, then, can be said of the present book? For the fan it will probably supply a number of his favorite stories in a convenient permanent form. For those new to the field it is perhaps not quite so likely to make converts as the previous anthologies, since the vein being worked is not a broad one and much of the richest ore has already been removed. It will, however, give the new reader a pretty fair picture of both the strengths and the weaknesses of this type of fiction. Thus, in "No Woman Born," by C. L. Moore, he will find a very excellent concept, and will also find how it can be diluted by verbosity and repetition. "With Folded Hands," by Jack Willi-son, on the other hand, is much more satisfactory in its presentation of a serious idea in fictional form. In it, as in most of the other successful stories, the emphasis on science is small (this despite the evolution mentioned above), little scientific explanation is attempted, and the fanciful inventions and far-off times and places are used primarily to widen the scope of human situations which the author can examine.

R. M. SCHR

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HEALTH FOR THE MASSES

You and Your Doctor. Benjamin F. Miller. 183 pp. $2.75. Whittlesey House. New Yo

THE author, a clinical professor of medicine with experience in medical research and the public health field, has given us an unusually readable book on a subject of vital concern to all. Attention is focused on the current major questions of medical care; for example: the general practitioner, "a casualty of modern science;" overspecialization; group medical practice; preventive medicine; emotional illness; medical check-ups; medical education and research; prepayment of medical care; and administration of national medical care. The *status quo* in medical care is repeatedly and forthrightly challenged, although generally in a temperate vein and with stimulating discussions of the changes deemed imperatively necessary. As regards the argument that today American health is excellent by comparison with previous years or other countries, the author retorts that "We should use only one yardstick to measure health. It must be based on the scale of what can be achieved at the present time" (p. 175), and he demonstrates that very much more indeed could be attained even now.

There is some confusion in the book about the relationship between group practice and prepayment, apparently an assumption of identity between the two (pp. 45, 123, 179). Actually, most groups are fee-for-service (about 80 percent of groups with five or more physicians, as of 1946). It is well to remember also that medical groups, prepayment or otherwise, merely approximate the ideal dealt with in the book—some more closely, others not so closely. Be this as it may, there can be no doubt that, in this simply told yet informative and provocative book, the author has made a notable attempt to acquaint the layman with the point of view of one important school of medical thought on ways and means of attaining optimum national health.

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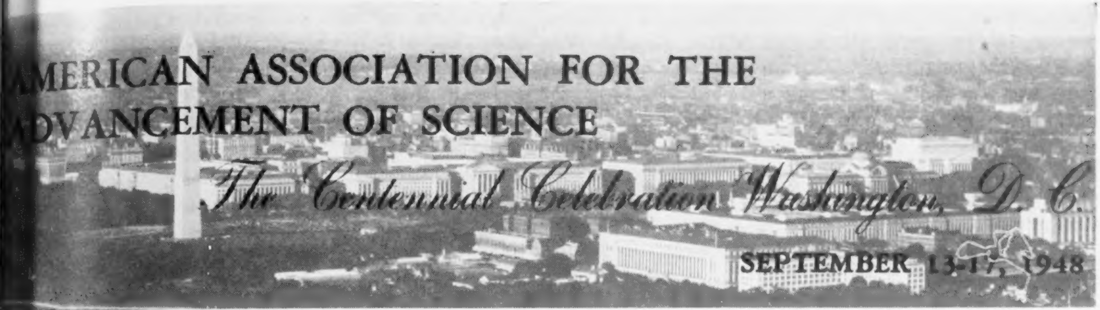

AAAS SUMMARIZED PROCEEDINGS AND DIRECTORY

It is a pleasure to report that the *Summarized Proceedings and Directory of Members* of the Association is now nearing completion and is scheduled to be ready for distribution in August. The long delay in its appearance has been due entirely to the difficulties of getting such books printed.

The forthcoming volume is more than a report on a century of activities of the Association. It includes corresponding information about the Pacific and Southwestern Divisions of the Association. Still further, it contains important statistical data about each of its 203 affiliated and associated societies: names and addresses of their secretaries, dates of organization, objects, membership dues, total memberships, divisions and branches, meetings, journals and subscription prices, prizes and medals, and libraries. Together these data present a summary of the present status of most of American organized science.

As a background for the activities of the Association and its affiliated and associated societies from the respective dates of their organization to the present time, the dates of the founding of the principal academies of science and universities throughout the world are recorded from the year A.D. 983 to about 1600. In order to illustrate how far the organization of scientific societies has advanced in the past hundred years, a review is presented of the attempts to establish them in this country in the decades immediately preceding the founding of the Association a hundred years ago.

Scientific progress has been so rapid and revolutionary during the life of the Association that it is difficult now to understand and appreciate the great adventures and achievements of our predecessors a few decades ago. To assist in the orientation of readers, the historical sketch of the Association is divided into five 20-year periods, and a few of the greatest scientific discoveries in each of them are enumerated. In these reminders of the great achievements of the past such names as Lyell, Darwin, Kirchhoff, Mendel, Mendelyeev, Hertz, Maxwell, Planck, DeVries, and Bateson appear.

The Directory of Members portion of the book (about 1,500 pages) contains approximately 40,000 names, alphabetically arranged. The name of each member is followed by the year of his birth, his address, the university from which he received his highest degree, his field of specialization or chief scientific interest, his professional position, the year he became a member of the Association, the year he became a Fellow, and the section or sections of the Association with which he is affiliated.

The volume closes with an unusual and valuable section, a Geographical Index of the names and sectional affiliations of all members of the Association at December 31, 1947. That is, the names of all members who are residents of the United States are grouped, first by states in alphabetical order, then by cities and towns within the respective states (also in alphabetical order), and, finally, the names of members in each city or town are arranged in alphabetical order. The names of members who are residents of foreign countries are similarly arranged alphabetically in sequence by continents, countries, cities, and individuals. The Association has members in 76 foreign countries.

It frequently happens that a librarian or scientist wishes to get the names of chemists, zoologists, or specialists in some other field of science who are residents of a particular city, such, for example, as Urbana, Illinois. With the new Directory before him he will turn to Illinois in the Geographical Index, and then to Urbana. Probably to his surprise, he will find that 77 members of the Association are residents of Urbana. To the right of each name is a letter indicating the sectional affiliation of the member—*C* for chemistry and *F* for zoology. By counting the *C*s and *F*s he will learn that, of the 77 members of the Association who are residents of Urbana, 33 are chemists and 29 are zoologists. If he should desire information about any chemist or zoologist in the list he would then turn back to the General Directory.

F. R. Moulton

SYMPOSIUM ON NATURAL RESOURCES

The Symposium on Natural Resources will deal, in a broad and diversified manner, with man's relationship to the useful materials in his environment. The aim of the contributions will be to clarify thinking by

the scientific and general public on the principles involved in a wise utilization of such resources. At the present time the difference between the renewable, or cyclical, and the nonrenewable, or noncyclical, resources of the earth is inadequately appreciated. The basis of any sound permanent economy must lie in ensuring that the renewable resources are really used in ways that permit full cyclic restoration, and that, in so far as nonrenewable resources are vital to such an economy, adequate provisions for continual exploitation of progressively poorer sources, and of substitute materials, be made continually in advance of exhaustion. Such substitutions involve social and economic as well as purely technological problems. The whole material basis of human culture is involved in problems of this kind. Such problems can be discussed only in terms of a wide range of disciplines, ranging from geochemistry and plant physiology to economics and social anthropology. Moreover, any practical consideration of the question as to whether we are making the best use of our resources inevitably involves problems of value in contemporary cultures. A particularly urgent case is involved in the extreme strain placed by modern warfare on the riches of the earth.

The contributors to the symposium will be T. S. Lovering, of the U. S. Geological Survey; Stanley A. Cain, of the Cranbrook Institute of Science; and G. E. Hutchinson, of the Osborn Zoological Laboratory, Yale University. Dr. Lovering has had extensive experience as an investigator and a teacher and has given much attention to the general aspects of mineral resources. He is a graduate of the Minnesota School of Mines and has a doctorate from the University of Minnesota. He is the author of *Minerals in World Affairs.* Dr. Cain is one of the ablest of the younger botanists of the United States. He is a graduate of Butler and has his doctorate from the University of Chicago. Before being called to the Cranbrook Institute he taught at Butler, Indiana, and the University of Tennessee. He has been particularly interested in the broader aspects of plant ecology and their relationship to evolutionary and general biological problems. Professor Hutchinson, who has given attention to these problems in connection with his work as consultant in biogeochemistry at the American Museum of Natural History, will discuss "Man in the Biosphere."

SYMPOSIUM ON SOURCES OF ENERGY

THE material progress of civilization depends largely on the energy that man can make available to do his physical work. The energy of the sun over past ages has been abundantly stored and conveniently packaged for our use, but we are using up these resources at an alarming rate. We know that the sun's energy comes from reactions of atomic nuclei and we have lately learned how to produce atomic energy under controlled conditions. We are not sure that mankind can be trusted with this newly discovered source of energy, but we must carry our research programs forward vigorously to determine how our resources of uranium and thorium can best be utili for peaceful purposes. Finally, we have the energy the sun with us always, but we are now utilizing o a small fraction of it. Sunlight falling on the ear surface does not give temperatures high enough to attractive for the direct production of useful pow Our most common method of utilizing this energy by means of a photochemical reaction in which car hydrates are produced that can then be burnt w the oxygen of the air to release this stored energy higher temperatures.

What are the facts regarding a greater utilizatio the sun's energy, and what are the chances that an creasing world population in its insatiable desire more energy can, in the future, obtain still more fr the sun?

Most of the energy running man's machines co from oil or coal or water power. In "Energy in Fossil Fuels," M. King Hubbert, associate dir tor of Exploration and Production Research, Shell Company, Houston, Texas, will discuss these th sources; he will review briefly the evolution of fos fuels and show that many geological ages are nece sary for nature to accumulate the fuel that man uses in a few generations. He will emphasize the fact th the exploitation of fossil energy is irreversible and th the historical events associated with it are witho precedent and incapable of repetition.

Mr. Hubbert will give a summary of the appro mate magnitudes and distributions of the vari classes of fossil fuels over the surface of the ear He will try to project the time sequences into t future, and will show that the rates of consumption these fuels after passing a maximum, or seve maxima, must ultimately decline asymptotically zero.

Speaking on "Atomic Energy," Eugene P. Wign of the Physics Department, Princeton University, w discuss the possibilities for the utilization of t uranium and thorium resources found in the ear Professor Wigner played an important part in t successful design of the nuclear chain-reacting pil at the University of Chicago, and in 1946–47 he wa research director of the Clinton Laboratory.

Farrington Daniels, of the Physics Departme University of Wisconsin, will speak on "Sol Energy." He will emphasize the very efficient way i which nature makes use of the enormous amounts energy that fall annually on every acre of the ear surface, for the production of carbohydrates and oth organic material in the growth of plants. He will sum marize recent developments and new laboratory e periments designed to understand better the mecha nism by which carbon dioxide and water are broug together by means of sunlight working through t agency of chlorophyll; he will also summarize what now known concerning the possible efficiency in t number of tons of carbohydrates that can be produ per acre per year with different kinds of plants. H will discuss some of the speculations that have been

rected toward a greater utilization of sunlight for the production of fuel and food, particularly in areas where the soil and water conditions are not conducive to agriculture. Finally, he will outline briefly other proposals that have been made for utilizing sunlight to supply the energy requirements of future civilizations after the fossil fuels have been exhausted. Professor Daniels was director of the Metallurgical Laboratory at the University of Chicago during 1945–46.

SYMPOSIUM ON WAVES AND RHYTHMS

ONE of fifteen symposia planned for the Centennial Celebration in Washington of the American Association for the Advancement of Science will deal with the subject of waves and rhythms. If nature ever abhorred a vacuum, she seems to love waves and rhythms. Rhythmic events are characteristic of many phenomena, ranging from social, political, and economic cycles through a variety of biological rhythms to radiation phenomena in chemistry and physics.

Three papers will be presented on September 14 dealing with selected aspects of this subject. James B. Macelwane, S.J., will consider rhythmic events in the earth's crust from the point of view of a seismologist. Dr. Macelwane is professor of geophysics, dean of the graduate school, and dean of the Institute of Geophysical Technology at St. Louis University. He is a member of the National Academy of Sciences and author of several books and many papers on seismology and related subjects.

Hudson Hoagland will consider aspects of biological rhythms with special reference to determinants of rhythmic activity in the nervous system. Dr. Hoagland is executive director of the Worcester Foundation for Experimental Biology, neurophysiologist at the Worcester State Hospital, and research professor of physiology at Tufts Medical School. He is the author of numerous publications dealing with aspects of neurophysiology and the physiology of time.

A third aspect of waves and rhythms will be discussed by Vern O. Knudsen, a student of the physics of sound and of supersonics, who has made outstanding contributions to the solution of problems in audition. Dr. Knudsen is professor of physics and dean of the graduate division of the University of California at Los Angeles. He has served as president of the Acoustical Society of America and in 1934 was awarded the thousand-dollar prize of the AAAS. In addition to many scientific papers, he is the author of *Architectural Acoustics* and *Audiometry.*

SYMPOSIUM ON PROBLEMS OF THE OCEAN

AMONG the dozen or more symposia to be held during the Centennial Celebration of the AAAS at Washington, September 13–17, is one on Problems of the Ocean. Oceanography, which includes the study of the physical, chemical, and biological conditions of the ocean and which overlaps allied fields such as meteorology, geology, etc., has made rapid progress in the past decade. These advances range from those that have immediate and important practical application to those that aim primarily at adding to the sum total of human knowledge. There is need, however, for a much better understanding of the phenomena of the oceans. This is emphasized when, in the present state of the world, these great bodies of water must be considered as connecting links rather than separating barriers, and when it is also clear that the rapid rate of increase in human populations requires an expanding dependency on marine resources, i.e., the resources of perhaps the only major area not yet fully exploited.

Early oceanographic expeditions obtained empirical information on currents, temperature, waves, and other phenomena. These followed notable advances in devising physical and chemical theories to explain the distribution of properties found in the ocean, but at the present time these fall far short of actually accounting for existing conditions. Theory and experiment will undoubtedly provide explanations and methods for predicting time changes and for extrapolating knowledge into the depths and other areas thus far essentially unexplored. This goal is still far in the future, however. Because of the complicated nature of the interrelationships that exist in the ocean, it is at present necessary in many cases to turn to empirical correlations to meet immediate demands.

Although fundamentally unchanging, the oceans are never at rest, nor is the distribution of properties ever twice exactly the same. Within limited areas and over short intervals of time, conditions change to a degree that exerts great influences on the organisms living in the sea, on the atmosphere, and on the coasts and sea bottom. Because of these facts the close interrelationship between the various phases of oceanographic research, and their mutual dependence on related fields of endeavor, have long been recognized.

The themes of the three speakers at the symposium on Problems of the Ocean represent the major bases on which the science of oceanography rests. Carl Eckart, director of the Scripps Institution of Oceanography at La Jolla, California, will discuss the theory of stirring and mixing; it is ordinarilly thought that stirring and mixing can occur only in turbulent motion, but closer examination reveals that relatively simple motions also result in stirring. Richard H. Fleming, Chief, Division of Oceanography, Hydrographic Office, U.S. Navy, will describe the requirements for surveys and the various applications of oceanographic information. Daniel Merriman, director, Bingham Oceanographic Laboratory, Yale University, will talk on the biological problems of the ocean. The symposium will conclude with comments from the floor by several principal discussants and members of the audience.

SYMPOSIUM ON GENES AND CYTOPLASM

ANOTHER of the fifteen symposia planned for the Centennial Celebration of the American Association for the Advancement of Science is to deal with genes and cytoplasm. The fundamental importance of the gene in all kinds of creatures from microorganisms to man is widely recognized; but *how* genes accomplish their great effects, control of the biochemical, physiological, structural, and behavioral properties of the organism, has only within the past decade been subjected to concentrated exploration on a scale and in a manner that may lead to a general solution. This broad question involves two distinct problems that have been attacked by different experimental methods. On the one hand is the problem of the nature of the primary activity of genes, their first, and possibly their only, direct activity. On the other hand is the problem of how these basic gene activities can result in the development of an organism with its cells and tissues alike in genes but different in hormone and enzyme production, in structure, and in behavior. The problem of abnormalities in development, such as cancer, is at least in part a special case of the more general problem of developmental differentiation. Not only are the diversities among cells of one body cytoplasmic, but the products of gene activity operate *in* the cytoplasm, and there appear to be, moreover, cytoplasmic materials (plasmagenes) comparable to nuclear genes. The cytoplasm must therefore be investigated along with the genes in attempts to discover what genes do and how cells with the same genes become diverse. These two major problems are dealt with from biochemical, genetic, and embryological points of view by the three papers to be presented in the Symposium on Genes and Cytoplasm.

One of the most fruitful trends in modern work has been to shift attention from morphological traits, which are remote from the primary action of genes, to biochemical properties of the organism, which must underlie the morphological traits and be more directly related to primary gene activity. Among biochemical traits, two classes have held out the greatest promise of being close to primary gene activity: enzymes and antigens. Indeed, it has even been suggested that the primary products of gene activity may be enzymes, antigens, or their specific reactive molecular groupings. In this field, no modern investigation has been more successful or attracted more attention than the biochemical genetics of the bread mold, Neurospora, a program initiated by the geneticist Beadle in collaboration with a group of biochemists led by Tatum. One of the initial members of this group, the biochemist David Bonner, of Yale University, will present a paper on the status of genic control of biochemical reactions and the possibility of arriving by such studies at a knowledge of the gene and its primary activity.

Whatever conclusions are reached as to the primary action of a gene, they will have to be reconciled with many facts discovered by the methods of genetics. Among these are the facts on which are based the concepts of position effect, interaction of alleles, and genic balance. The activity of a gene may be modified either in dependence upon its position in the chromosome, or by the kind of allele present in the same nucleus, or by the ratio between the number of genes of one kind and the number of genes of other kinds present in the nucleus. These phenomena, puzzling in themselves quite aside from the difficulty of reconciling them with the rapidly growing knowledge of biochemical genetics, form the subject of a paper to be presented by Curt Stern, of the University of California at Berkeley. Professor Stern has discovered new facts in these fields and interpreted them by theoretical concepts to account for these difficult matters. These concepts lend themselves readily to a reconciliation between them and the new biochemical genetics.

The biochemical approach represented by Bonner and the genetic approach of Stern go far toward providing us with insight into the primary activity of genes. There remains, however, the essentially embryological problem of accounting for the origin of differences among cells with the same genes. Here a decisive role of cytoplasmic factors has long been suspected. In agreement with this, German plant geneticists have for over 20 years been accumulating evidence for the existence in the cytoplasm of genelike determiners. Similar agents have more recently been discovered in a few animals by French, American, and English workers. Curiously enough, though the problem is essentially an embryological one, the main clues to its solution have come from studies on unicellular organisms, in which all the cells produced by one ancestor are usually alike in traits as well as in genes; yeast, bacteria, and the protozoan *Paramecium*. It is no accident that the traits investigated in these organisms are mainly enzymes and antigens, the two classes of substances held to be most closely related to gene activity. An account of this work will be presented by T. M. Sonneborn, of Indiana University. The work to be reviewed by Professor Sonneborn leads to the view that the products of gene activity are themselves endowed with the genic property of self-duplication and that competition among these plasmagenes gives rise to persistently diverse cell types.

With this full turn of the wheel, there emerges the beginning of a synthesis. Each gene performs a definite biochemical activity, probably conferring specificity upon the more complex materials of the cell, such as enzymes (Bonner); in this activity, the genes vary in their affinity for the substrates on which they operate, and then vary in their efficiency in operating upon them (Stern); the enzymes and antigens specified by gene activity may themselves be genelike (plasmagenes), and the diversities arising among cells with the same genes may be due to the different results of competition among them under different conditions (Sonneborn).

The SCIENTIFIC MONTHLY

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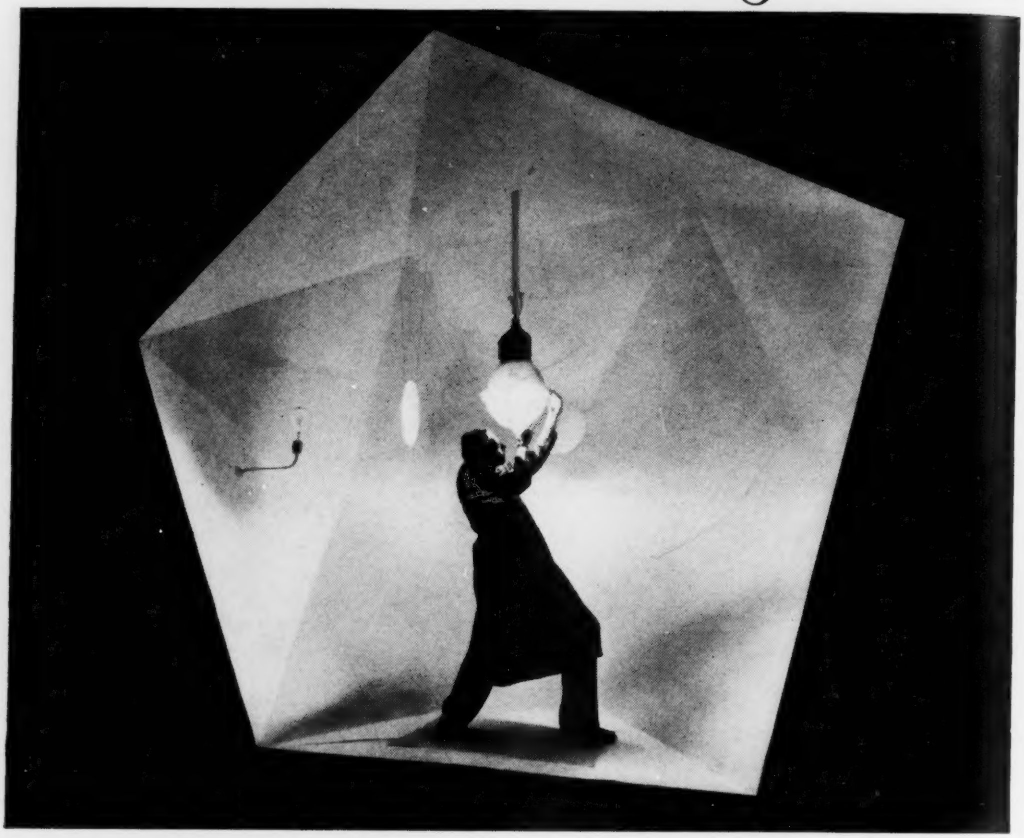

THE THOUSAND DOLLAR PRIZE

The First Thousand Dollar Prize for "a Notable Contribution to Science" was established in 1923 by a friend of the American Association for the Advancement of Science who wishes to remain anonymous. Beginning in 1923, the Prize has been awarded at each annual meeting of the Association, in recognition of the importance of scientific research reported for the first time before some scientific session at a general meeting of the Association. It is not necessary that a paper considered for The Thousand Dollar Prize shall have been presented by a member of the Association, but, to be eligible for consideration, it shall not have been an official address or an invited paper. It is the wish of the donor of the Prize that younger scientists shall be particularly considered in making the award.

Thirteen of the prize winners have written articles for our Centennial Issue. Their contributions appear in the pages that follow. Other recipients of The Thousand Dollar Prize, not included in this issue, are:

1923	* Leonard Eugene Dickson (mathematics)
1924	Edwin P. Hubble (astronomy) (shared equally with L. R. Cleveland)
1925	* Dayton C. Miller (physics)
1926	* George D. Birkhoff (mathematics)
1927	H. J. Muller (genetics)
1928	Oliver Kamm (chemistry)
1934	Vern O. Knudsen (physics)
1935	P. W. Zimmerman (plant physiology) and A. E. Hitchcock (botany)
1936	W. M. Stanley (chemistry)
1939	I. I. Rabi (physics)
1941	D. E. S. Brown (physiology) (with F. H. Johnson and D. A. Marsland)
1947	Harrison Brown (physical chemistry)

* Deceased.

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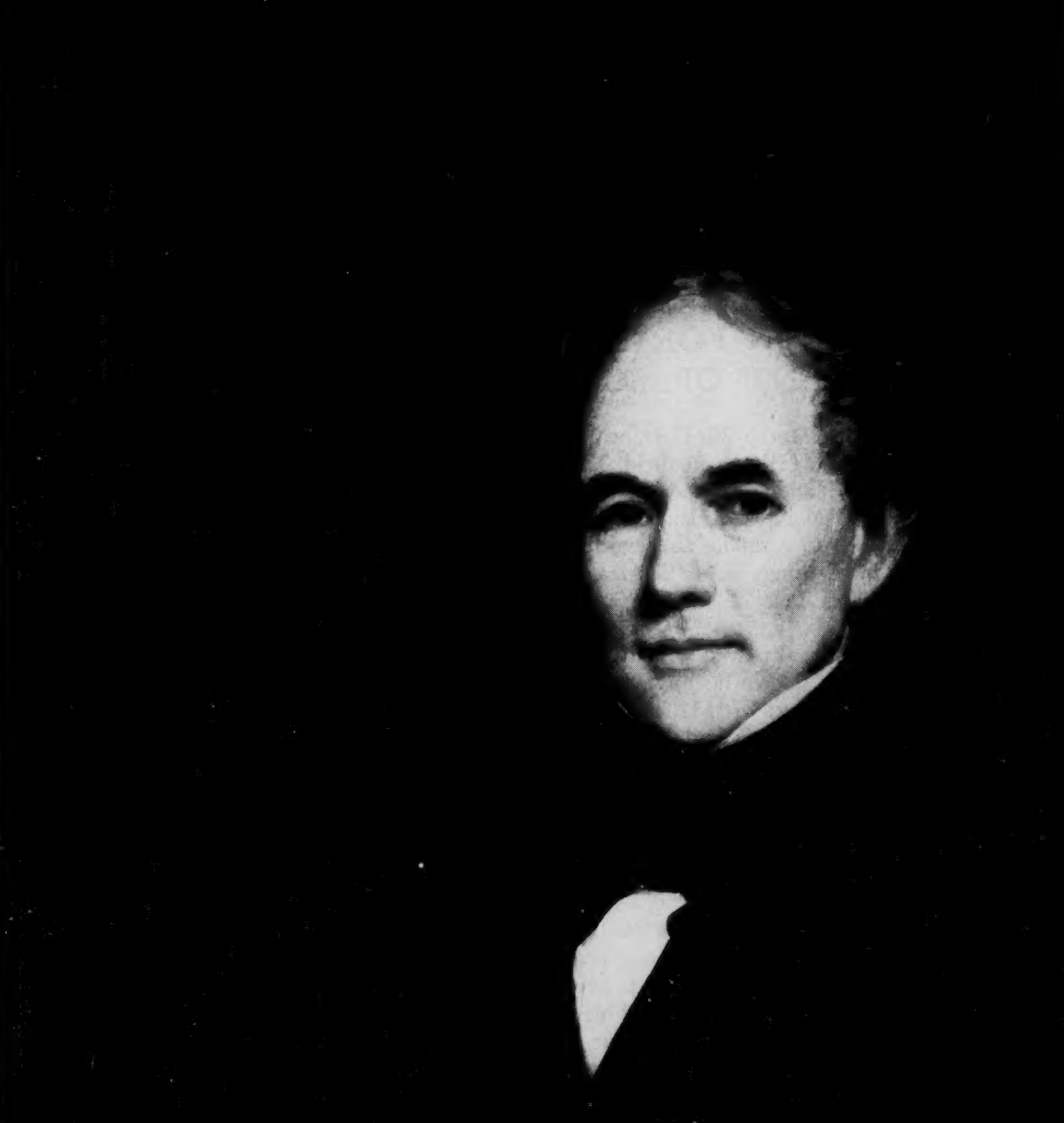
THE FIRST PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

William C. Redfield was born March 26, 1789, at Middletown, Connecticut, the eldest of the six children of Peleg and Elizabeth (Pratt) Redfield. To avoid confusion with two other William Redfields in his vicinity, he himself added the middle initial C to his name. It stood, he was wont to say, for "Convenience." Peleg died in 1802, leaving the family in straitened circumstances. William was apprenticed at thirteen to a saddle and harness maker of Upper Middletown (now Cromwell), Connecticut. In the evenings he studied science by the light of a wood fire. His chief inspiration came from Dr. William Tully, a local physician, to whose library young Redfield had free access.

His mother remarried in 1802 and moved with her husband, his nine children, and her four youngest to Portage County, Ohio. When his apprenticeship was over, William set out on foot, over primitive forest trails, to visit her. In 1811 he tramped back along a more southern route to Middletown, where he went to work at his trade. He also ran a small store. Every possible spare moment he gave to the study of science. On September 3, 1821, came the "great September gale;" shortly afterward, on a trip to western Massachusetts, Redfield concluded, from the lay of the trees felled by the wind and the times of the storm's occurrence at various places, that the storm had been a progressive whirlwind. In 1831 he brought this fundamental concept of the nature of such storms to the attention of the public, in an article in the *American Journal of Science and Arts*. His paper in the October 1833 issue of the *Journal* is a meteorological classic (*Observations* on the *Hurricanes and Storms of the West Indies and the Coast of the United States*). He put his knowledge of hurricanes to work by devising a set of practical rules by which the mariner could know where he was in such a storm and what to do to avoid its dangers.

Redfield's interests were many. In 1820 he turned his attention to steamboat navigation. Travel by boat had fallen off owing to a number of disastrous explosions. Redfield put into successful operation between New York and Albany a line of "safety barges." These barges, or passenger boats, were towed behind a steamboat at a sufficient distance to be safe in case of a boiler explosion. After the public went back to the cheaper and faster, if less safe, mode of travel, the barges were used to carry freight. After more than one hundred years this method of river shipment is still in use. Redfield was also interested in railroads, and in 1829 published a pamphlet on a proposed railroad to connect the Hudson and the Mississippi rivers (*Sketch of the Geographical Rout* [sic] *of a Great Railway* . . . , 1829). He urged the construction of other railroads, among them one from New York to Albany, in spite of the fact that he was financially interested in travel and shipment between these points by boat and barge. In 1848 he was elected the first President of the American Association for the Advancement of Science, an organization he had helped to found, and presided at its meetings in September of that year. Redfield died in New York City on February 12, 1857.—Abridged from the *Dictionary of American Biography*.



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THIRTY YEARS OF MASS SPECTROSCOPY

ARTHUR J. DEMPSTER

Dr. Dempster, Argonne National Laboratory, Chicago, received the Annual Thousand Dollar AAAS Prize in 1929.

N THIS anniversary year of the AAAS it is fitting that we should review the development of mass spectroscopy. It is now nearly three ades since many of the ordinary chemical elents were found to be made up of isotopes, that of atomic forms that are chemically identical, but fer in mass and other nuclear properties. At close of the first world war the theory that atom had a central nucleus was well established. made the charge on the nucleus the basis for the mical properties of an element, and thus left en the possibility of several nuclei of different ss having the same charge. The naturally radioive substances had just been satisfactorily arged as groups of isotopes. Thus the discovery isotopes in the ordinary elements fitted readily o the newly developed nuclear theory of atomic ucture.

Mass spectroscopy is the study of these isotopes d their combinations and is based primarily on identification of the mass of a charged atom or lecule by means of its deflection in a magnetic d. The subject grew out of the earlier study "positive," or "canal," rays. These rays appear en an electrical discharge passes through a gas a low pressure, and were found to consist of itive ions which were attracted to the negative ctrode. If there is an opening in the electrode, rays pass through with a high velocity and exd as a luminous bundle into the space behind. ese positive, or canal, rays affect a photographic te, cause fluorescence on a screen, or charge up electrode in their path. Such a beam of positive atoms is deflected when it passes through a magnetic field; the amount of the deflection varies according to the mass, or atomic weight, of the moving particle, and thus serves to analyze the atoms present in the beam. Several methods of using this principle of magnetic analysis will be outlined in this article.

Mass spectroscopy is primarily the scientific study of isotopes and the ions made from them, but there has been an ever-widening application of its results and methods to chemistry, biology, and industry. The scientific side includes three basic problems: what stable and radioactive isotopes exist in each element; what is the relative abundance of the stable isotopes; and what are the exact masses of the isotopes.

Naturally occurring isotopes. In a series of investigations between 1907 and 1913, Sir J. J. Thomson had succeeded in distinguishing atoms and molecules differing in mass by 1 part in 15, and in showing that neon was probably made up of two kinds of atoms with different masses. Improved methods of analysis were devised in 1918–20 that were characterized by the use of focusing —the naturally divergent beam of charged atoms was made to recombine after the magnetic analysis. The advantage thus gained was analogous to that obtained by using a lens—a great increase in intensity and definition in the images—with the result that the isotope masses of even the heavy elements could be separated and measured. In the succeeding decades these improved methods were

extended to all the chemical elements and showed that in most cases they had a complex isotopic structure.

The exploration of the isotopic constitution of the naturally occurring elements was the main activity in this field during the first half of our thirty-year period. By 1933, 183 isotopes had been identified in 66 elements. In the second fifteen years this number was increased to 283 in the 83 naturally occurring elements. Of these, no less than 202 in 71 elements were first found by F. W. Aston. The experimental arrangement used by Aston illustrates the method used in this analysis and is shown in Figure 1. The source of ions was the gas discharge at the left in which positively charged atoms and molecules were formed and ac-

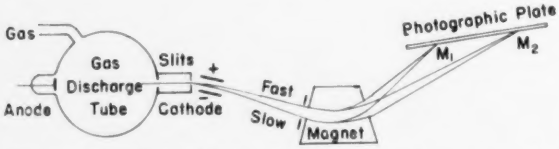


Fig. 1. Focusing of fast and slow ions from gas discharge (Aston, 1920).

celerated toward the cathode, thus acquiring various energies—corresponding to 20,000–50,000 volts. A narrow ribbon of moving particles was deflected by an electric field, the slow ions being deflected more than the fast. In the magnetic field which followed, light masses M_1 were more deflected and struck the near end of the photographic plate, whereas the heavy masses M_2 gave an image at the far end. The particle paths shown resulted in the fast and slow rays of each kind being focused on the photographic plate. An example of such a photographic mass spectrum showing seven isotopes is reproduced in Figure 2.

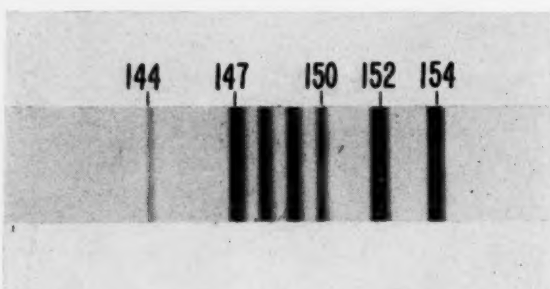


Fig. 2. Mass spectrum of normal samarium, showing seven isotopes.

Another focusing arrangement, illustrated in Figure 3, was used by Dempster in 1918 to analyze ions liberated from heated salts, and in 1921 to analyze ions of gases and vapors produced by electron impact. This focusing arrangement ma use of the fact that semicircles of the same ra passing through a point come together after 18 as shown in Figure 3. This arrangement ha

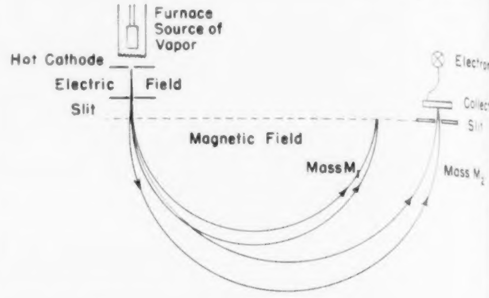


Fig. 3. Focusing of a divergent ion beam with s energy (Dempster, 1918).

proved to be especially useful for measuring t abundance of isotopes and analyzing the prod produced by electron ionization. Owing to the d ficulty in devising suitable sources of ions for s elements, many of the isotopes were not discover till late in the thirty-year period. The spectrogra illustrated in Figure 4 was constructed by Dem ster in 1935 and, using a spark to produce io made possible the identification of 33 new isotop in 17 elements.

Seven of the 283 naturally occurring isotop were first detected by the analysis of optical spec (C^{13}, N^{15}, O^{17}, O^{18}, Si^{30}, In^{113}, and Pb^{204}). Evap ration of liquid hydrogen at a low temperatu served to concentrate the heavy hydrogen isoto of mass 2 to a point where it could be detec spectroscopically. The helium isotope of mass 3 w detected by the cyclotron and is at present t least-abundant stable isotope known to exist nature, since it is approximately a million times le abundant than ordinary helium. Several physicis particularly Bainbridge and Nier, using ma spectroscopic methods, added various isotopes complete the present list.

The discovery that the chemical elements we made up of isotopes with nearly integral ma values led to the revival of Prout's theory of 181 according to which the atoms of all elements a different aggregations of hydrogen atoms. Th fundamental units in the nuclei were at first co sidered to be protons and electrons, but after th discovery of the neutron in 1932 and the develo ment of the theory of nuclear "exchange forces the building blocks were supposed to be proto and neutrons, the number of protons being equ to the nuclear charge and the total number of pr tons and neutrons being equal to the isotopic mas

The binding forces holding these particles t

gether are still a subject of investigation. Recently the discovery of mesons has introduced new elements into the theory of nuclear forces. To quote one theorist, "These forces, in which the neutral or electrically charged mesons play the role of photons, and the nuclear particles the role of sources of these heavy quanta, are still not known in detail, much less understood."

Among the 283 naturally occurring isotopes, several are of special interest. Five radioactive ones occur among the stable isotopes, K^{40}, Rb^{87}, Sm^{152}, Lu^{176}, and RE^{187}, and the fact that they still exist bears on the question of the age of the earth. Recently small amounts of the relatively short-lived radioactive carbon isotope C^{14} have been found in organic matter. Four stable isotopes of elements useful in biological studies have now been separated by

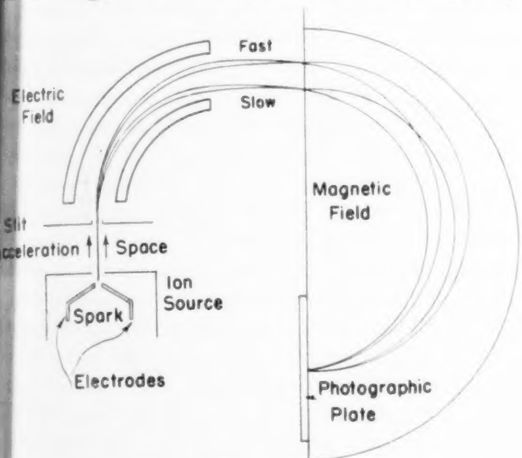


Fig. 4. Path of rays from spark source in a double-focusing mass spectrograph (1935).

the methods of physical chemistry, so that the hydrogen isotope of mass 2 (deuterium), carbon of mass 13, nitrogen of mass 15, and oxygen of mass 18 can be obtained by investigators. These may be built into organic molecules, or even into known parts of complex molecules, and be made to show the details of physiological or other processes, since they can be identified later by the mass spectrometer. Although the isotopes of an element, as the name signifies, are almost identical in chemical behavior, they may differ enormously in nuclear reactions; for example, some isotopes have a very much greater power of absorbing slow neutrons than their neighbors, the outstanding ones in this respect being B^{10}, Cd^{113}, Sm^{149}, Gd^{153}, Gd^{155}, Hg^{196}, and Hg^{199} (Figs. 2, 5). The mercury isotope of mass 196 has a very small abundance, but its absorbing power is so large that 5 percent of the neutrons absorbed form radioactive mercury atoms of mass

Fig. 5. Abnormal abundance of samarium isotopes produced by neutron absorption in isotope at 149.

197, which after a few hours become ordinary gold. In a nuclear reactor we can make gold in appreciable quantities and thus finally realize the dreams of the alchemists.

The rare isotope of uranium, U^{235}, found in 1935, is also of special interest. It is worth noting as characteristic of the essentially glacierlike advance of basic science (popular opinion notwithstanding) that this very significant entity was not noticed until after fifteen years of active exploration in this field, and only after 200 other isotopes had been discovered. A photograph showing the doubly charged uranium isotopes is reproduced in Figure 6.

At first this isotope was of interest to the radiochemists because it satisfied all the requirements for the hypothetical "actino-uranium," the origin of the actinium series of radioactive substances. After the discovery of the fission phenomena in uranium, the fundamental scientific question arose as to whether the faint isotope was responsible for the effects observed. A few micrograms of uranium were separated in 1940, and the isotope at mass 235 was found to be especially susceptible to fission. This discovery led to projects for separating large amounts of the isotope.

Radioactive isotopes. Radioactive isotopes were identified in the naturally radioactive elements as early as 1906, and the chemical identity of several pairs and groups of radioactive substances was

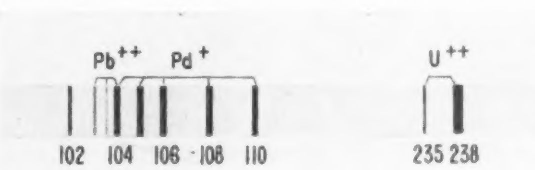


Fig. 6 Mass spectrum of uranium showing the faint isotope of mass 235. The palladium is from one electrode and served for calibration (Dempster, 1935).

well established in 1913 when the term isotopes was first introduced. Between lead and uranium 40 different kinds of radioactive atoms were known, and these fell naturally into 10 groups with 2–8 isotopes in each. The isotopes in each group were identical chemically, but presumably differed in mass.

The observation in 1934 by I. Curie and F. Joliot that alpha-ray bombardment of boron and aluminium gave rise to radioactive nuclei with an appreciable lifetime was the first item in an unexpected extension of our knowledge of isotopes. This was brought about by the discovery of many new methods of causing transformations. The cyclotron came into operation at this time and produced protons and deuterons of high energies; other methods of accelerating these particles were developed; the neutron was discovered and found to produce transformations in nearly all nuclei. The number of radioactive isotopes grew rapidly, and by 1943 more than 400 so-called active isotopes had been observed. Their masses not only occupy the gaps between the stable isotopes, but extend out to lighter and heavier masses. Chlorine, for example, has 2 stable isotopes at masses 35 and 37, and 4 radioactive isotopes at masses 33, 34, 36, and 38. These unstable atomic structures, usually called "radioisotopes," resemble each other and the stable isotopes of the same element in chemical properties only. In nuclear properties the radioactive isotopes differ radically not only in the radiations they emit but in the characteristic rates at which they change into stable nuclei. In some cases two radioactive isotopes called "isomers" may have the same mass, or one may have the same mass as a stable nucleus. After the discovery of the fission of uranium, the study of the radioactive fission products led to a new increase in the number, so that at the present time we know approximately 540 different radioactive isotopes that may be produced by various means.

The masses of about half of these can be deduced from the methods used to produce them, and among the remainder the mass spectrograph has been used to determine their mass in more than forty cases. For this purpose the radioactive atoms were analyzed in the same manner as stable isotopes, being collected on a gelatin film and detected by means of their radioactivity, since they are usually far too few for the ordinary detecting methods (Fig. 7). However, with the formerly "missing" element of nuclear charge 43, technetium, the long-lived radioactive isotope at mass 99 could be observed directly by means of the charge it carried.

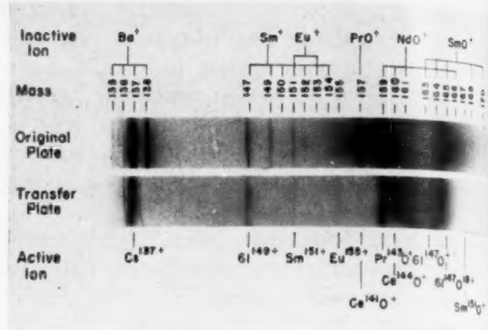


Fig. 7. *Above:* Mass spectrum of mixture of rare earth elements exposed to neutrons; *below:* transfer made by radiation from the radioactive isotopes (Lewis and Hayden, 1946).

Radioactive forms of carbon, sodium, phosphorus, calcium, iron, copper, and iodine are especially useful for biological studies, but unfortunately none of the radioactive isotopes of nitrogen or oxygen have a half-life longer than ten minutes. Consequently, for many investigations in physiology and organic chemistry, it is necessary to use the stable isotopes of these elements at masses 15 and 18 and to depend on a mass spectrometer for their detection.

The plutonium isotope of mass 249, made by neutron absorption in uranium, was discovered in 1941 and, like U^{235}, was found to be highly fissionable. This discovery transformed the leisurely investigation of the possibility of a nuclear chain reaction as a source of power into the wartime development of nuclear reactors as sources of neutrons for the production of this isotope.

Relative abundance of the stable isotopes. Twenty-two of the naturally occurring elements have only 1 isotope, 20 have 2 isotopes, and the remaining 39, all even-numbered elements, share the remaining 221. It is remarkable that no two patterns of abundance are alike, and that no general theory exists at present to explain how the different elements should have acquired the particular isotope abundances they now have. For most elements, the isotopic abundance was deduced from the density of images on photographic plates. For some elements, a more accurate measurement has been made by comparing the total charges carried by the different isotopes. The term "mass spectrometer" is applied to an instrument in which the abundance of the various ions is measured electrically; instruments that record the separated ions photographically are usually referred to as "mass spectrographs."

Slight differences have been found to occur in

the relative abundances of the isotopes of carbon, oxygen, boron, and lead. The carbon isotope at mass 13 is more abundant in organic material (1–[illegible]3) than in limestones (1–92.3). The heavy oxygen isotopes have a different abundance in different limestones, depending on the temperature at which the calcium carbonate was laid down (Urey, 1948). Water from different sources varies slightly in density, owing to variations in the relative abundance of the oxygen isotopes (Dole, 1936): in fact, it is difficult to define a standard sample of oxygen for atomic weight comparisons beyond a certain accuracy. Lead from different regions varies in isotope abundance, probably owing to the admixture of lead of radioactive origin (Nier, 1938).

Radioactive transformations and neutron absorption can give rise to very abnormal isotope abundances. Thus, strontium is formed in certain micas from the radioactive decay of rubidium of mass 87, and consists almost entirely of 1 isotope. Very abnormal abundances have been produced in cadmium, samarium, gadolinium, and mercury, which have isotopes with very large absorption for neutrons. These isotopes capture neutrons and increase their mass by unity. The abnormal abundance produced in samarium is illustrated in Figure 5, which should be compared with the normal distribution of Figure 2.

Masses of the isotopes. The discovery by Aston in 1920 that the mass of the hydrogen atom was 1.008, slightly greater than unity, when the light oxygen isotope is taken as exactly 16, was of fundamental significance for theories of nuclear structure. The existence of isotopes with nearly integral mass values leads us to think of the constituents of the nucleus, and if it is built up out of hydrogen nuclei—that is, protons—or out of protons and neutrons, the mass of a nucleus is not the same as the sum of the masses of its constituents. The doctrine of the conservation of mass in all combinations and reactions, though still fundamental in chemistry, had, however, been modified by physicists long before 1920. In 1881 J. J. Thomson showed that a body should acquire added inertia when it is charged, and this "electromagnetic" theory of mass was used in 1902 to account for the increase in the mass of beta particles with increasing energy. This extra mass for transverse acceleration, according to Lorentz' theory, was the kinetic energy divided by the square of the velocity of light. Since neutral atoms could be ionized to form charged particles, there were speculations that all mass might be electromagnetic in origin. That light radiation should be accompanied by a transfer of mass was shown by Einstein in 1905 to be required by the relativity principle when applied to the electromagnetic theory of light. The same formula $E = Mc^2$ for the mass transferred M by an amount E of light radiation also followed, however, from simply considering the pressure exerted by light on an absorbing surface and applying the laws of conservation of energy and momentum.

Popular writers have sometimes assumed that the relativity theory predicted that the formula $E = Mc^2$ would hold for any new forms of energy or processes that might be discovered. Commenting in 1907 on the generality of this formula, Einstein wrote:

> The circumstance that the general law is deduced from a special case makes it necessary to examine the necessity of the deduction. We ask: "Might not other special cases lead to other conclusions?" A general answer to this question cannot be given at the present time inasmuch as we do not have a complete theory of the physical world corresponding to the relativity principle. We must limit ourselves to special cases which we can handle at the present time by the relativity theory of electrodynamics.

Mechanics was later incorporated in the relativity theory. One simple way of doing this made inertia appear as an effect of energy, just as electromagnetic momentum and light pressure had been an effect of electromagnetic energy, and in this formulation the relation $E = Mc^2$ also held.

At present, however, theoretical physicists recognize great difficulties in reconciling quantum phenomena with classical electrodynamics. They are now speculating on radically new types of electron theory, in one of which, for example, the electron has an infinite electromagnetic mass, but includes a term which gives the correct change when the electron is bound in the hydrogen atom. Coming to the nucleus and its constituents, we are still further from a satisfactory theory of the meson forces, which presumably hold together the many particles in a nucleus and thus determine its binding energy and its mass. That the differences between the atomic weights of the elements and exactly integral values might be attributed to different binding energies was first suggested by Langevin in 1913, and, in several cases of nuclear transformations among the light elements, experimental observations have in fact shown that changes in nuclear energy E are reflected quite accurately in the changes in mass M as given by the formula $E = Mc^2$.

Although the artificial disintegration of several nuclei by alpha rays was first observed by Rutherford in 1919, this discovery led to no immediate comparison of the energy set free in these proc-

esses with the change of mass—partly because protons of different energies are formed and the nuclei left in various excited states, but also because the masses of the isotopes involved were not accurately known at that time.

The discovery by Cockcroft and Walton in 1932 of the artificial disintegration of lithium by protons was a great stimulus to mass spectroscopy. For the first time physicists had a nuclear transformation for which they could measure the energy set free in a nuclear process and compare it with a change of mass. It was at once apparent, however, that, although the directly observed energy change could be measured with comparatively great accuracy, the uncertainties in the masses involved (hydrogen, helium, and lithium) amounted at that time to 40 percent of the change in mass resulting from the transformation. To have the mass differences match the energy measurements in accuracy, it would be necessary to determine atomic masses to four or five decimal places, a precision approaching that of optical spectroscopy, and a hundred times greater than that previously attained. Under this stimulus the following decade, 1932–42, brought great progress in the precise measurement of atomic masses. Bainbridge in 1933 redetermined the masses of 11 light isotopes, including those of lithium, by using a "velocity filter." This selected a group of ions in a narrow velocity range and gave sharper images on the photographic plate; but still the uncertainty in the masses involved in the transformation of lithium amounted to 30 percent of that predicted by the mass-energy formula.

In the years 1935–40 several spectrographs of a new type were developed—based on the principle of so-called double focusing. The paths of the ions in a spectrograph of this type are illustrated in Figure 4. The ions were formed by a vacuum spark and were accelerated and deviated first by an electric and then by a magnetic field. The differences in the deflections produced in the fast and slow rays by the electric and magnetic fields were arranged to cancel, and divergent rays also recombined at the photographic plate; hence, the term double focusing. The advantage in this arrangement is analogous to that obtained in optics by the use of an achromatic lens, which recombines light rays differing in color as well as in direction. This instrument was initially used to analyze the ions produced by a spark in order to complete the preliminary study of the isotopic constitution of the elements. Incidentally, the spark source was also found to give a great number of multiply charged ions of the solid electrodes for which no other source of multiply charged ions was available.

Some of the most accurate measurements of the masses of the light elements were made by Bainbridge and Jordan in 1936 by means of the mass spectrograph shown in Figure 8; masses differing by as little as 1 part in 10,000 were completely separated. The lithium mass was remeasured, and the decrease in mass in the transformation by protons was found to agree quite accurately with the value computed from the mass-energy formula. In

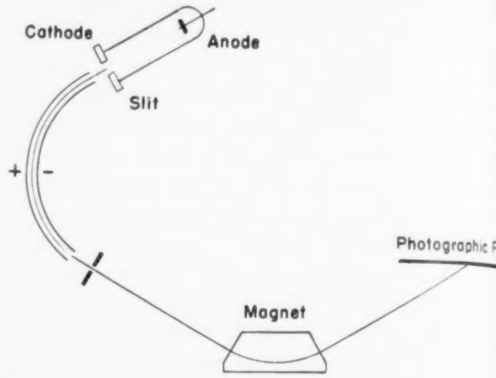
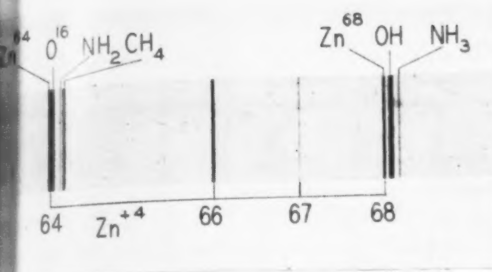


Fig. 8 Precision mass spectrograph for gas ions (Bainbridge and Jordan, 1936).

the same year Mattauch and Herzog in Austria and Aston in England also completed instruments that gave accurate mass measurements of the light isotopes. Similar observations were carried out in Japan in 1939, and at the University of Illinois by Jordan in 1940. In these latter measurements masses differing by only 1 part in 30,000 were completely separated. This marked an increase in accuracy of two hundred times that attained with Aston's first instrument, with which in 1920–22 he demonstrated the nonintegral nature of the hydrogen-oxygen ratio and the general shape of the packing fraction curve.

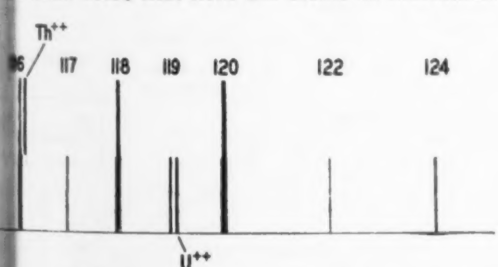
WITH the greater accuracy made possible with these instruments, the masses of 17 light isotopes were measured, and in the meantime many nuclear transformations had been found among these isotopes. Many of these transformations involved the emission of beta rays with energies that varied over a wide range, thus making it impossible to apply the formula $E = Mc^2$ to calculate an expected mass difference between the isotopes. It was, however, possible to maintain the formula by postulating a new entity, the neutrino, of very small mass, which carries off the energy difference between that actually in the beta ray and a constant maximum amount.

Apart from the 17 mass measurements just



g. 9. Zinc ions with four charges compared with other ns at masses 16 and 17.

ferred to, we have also at present approximately xty comparisons between the heavier stable isopes. These show several points of interest even ough the accuracy is less than that attained with e light elements. Taking the lightest oxygen isope as exactly 16, the masses of the isotopes dert from exactly integral values by various ounts. The fraction given by the difference vided by the integral value, amounting to a few rts in 10,000, is called the "packing fraction," and lues have been measured for many of the isopes. Figure 9 illustrates the mass spectrum of nc with 4 charges superimposed on several ions hose masses are very accurately known. Unfortunately, such direct deductions of packing fractions ve been made for only a few isotopes. Figure 10 ustrates a comparison between doubly charged otopes of uranium and thorium with singly arged tin isotopes. From sixty such comparins it is possible to draw a packing fraction rve as shown in Figure 11, which, although ot unique, does pass through values that are in cord with the measurements. If protons and neurons are taken as the nuclear constituents, the nding energies are very large; per particle the ergy amounts to several million electron volts as own by curve B in Figure 11. These very large nding energies indicate that the constituents of uclei are held together by very much stronger rces than those that hold the atoms in molecules.



ig. 10. Mass spectra of uranium and thorium with two harges superimposed on singly charged tin isotopes Dempster, 1936).

The mass changes in two nuclear phenomena, natural radioactivity and nuclear fission, are of special interest. Uranium of mass 238.13, after a series of radioactive transformations, finally becomes lead of mass 206.05. Subtracting only the masses of particles emitted, we would expect the mass of the lead to be 206.10. However, extra mass is lost and the energy equivalent of this extra mass using $E = Mc^2$ agrees very roughly with the total energy of the particles emitted in the series of transformations.

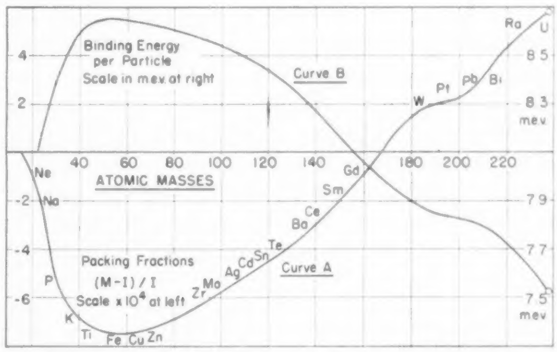


Fig. 11. Curve A, packing fraction curve; curve B, binding energy per particle in nucleus.

The mass differences shown in Figure 10 suggest a comparison with the energy released in the fission of a uranium or a thorium nucleus. In a more common type of fission, for which the masses may be taken from Figure 10, the uranium nucleus of mass 235.12 absorbs a slow neutron of mass 1.008 and produces as fission products columbium (92.944) and praseodymium (140.958). Two neutrons may also be formed (2.016) and, necessarily, 8 electrons (0.004). The total mass decrease is thus 0.211, and we would expect, if the mass-energy relation $E = Mc^2$ holds, that an amount of energy given by $0.211 \times 9 \times 10^{20}$ ergs should be released in the fission of 235 grams of the uranium isotope. In other units this is 20,000 kilowatt hours per gram, or 196 million electron volts (Mev) per atom for this particular method of fission. About 22 Mev remain for a time in the excited nuclear energy states of the fission products, and the remainder is released as kinetic energy of the nuclear fragments. That these nuclear fragments do have large kinetic energies was readily demonstrated by many experimenters in the spring of 1939, but the ranges of the fragments or the sizes of their ionization pulses do not supply us with accurate values of the energies of the particles. Direct calorimetric observations of the heat developed gave a rough agreement with the value computed above, but at

the present time we do not have accurate measurements of this interesting quantity and do not know how accurately the formula $E = Mc^2$ applies to this phenomenon.

Applications to chemistry, biology, and industry. Over a period of several decades the methods devised in one field of science have usually found applications in others, and this has been the case for mass spectroscopy. The method of analysis illustrated in Figure 3 was adapted very early in our period to the study of the kind of ions produced by the impact of electrons of various energies. In 1922–30 H. D. Smyth and others, by gradually reducing the electron velocity, found the minimum potential at which particular ions are first produced by electrons, and secured important data for the development of the quantum theory of molecular structure. In 1933–38 Professor Tate and others observed the patterns of singly and multiply charged ions that are produced in various gases by

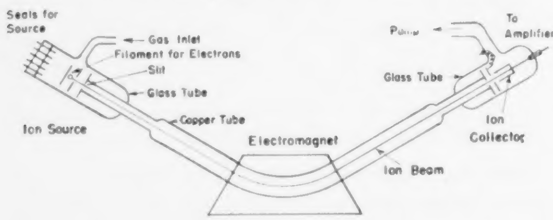


Fig. 12. Mass spectrometer for analysis of ions from gases (A. O. Nier, 1940).

electron impact. This application of mass spectroscopic techniques served as a basis for a new method of chemical analysis, whose importance in special fields is just beginning to be realized. The impact of 72-volt electrons on the benzene molecule, C_6H_6, for example, gives rise to 45 different positive ions with definite relative abundances. Conversely, the ion pattern can be used to identify the molecule, and the pattern observed with a mixture of a dozen or more hydrocarbons can be analyzed and ascribed to the superposition of the standard patterns from particular hydrocarbons. Instruments for carrying out these analyses in the oil and rubber industries were developed in 1939, and at the present time more than 30 such instruments are in continuous use in this country.

The mass spectrometer illustrated in Figure 12 was developed by Nier in 1940; it has recently been adapted for biological investigations, in which carbon of mass 13, nitrogen of mass 15, or heavy oxygen isotopes are used as "tracers." In such studies carbon dioxide, for example, may be made

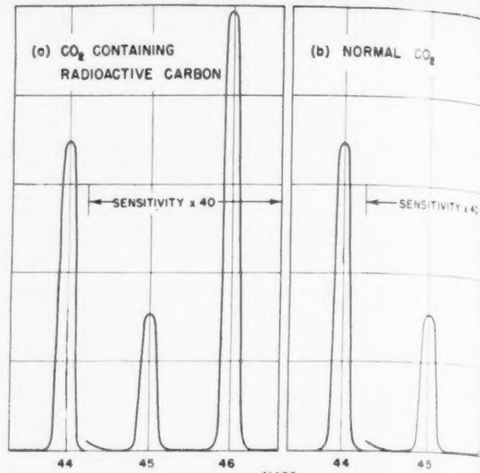


Fig. 13. Mass spectrometer record of ions from ca dioxide. Mass 44 is due to $C^{12}O^{16}_2$, mass 45 is du $C^{13}O^{16}_2$, and mass 46 is due to $C^{12}O^{16}O^{18}$ in (*b*) an this ion plus $C^{14}O^{16}_2$ in (*a*) (Ingraham and Hess, 1

with the heavy carbon isotope and built up by zymic conversion in tissues into complex orga molecules. The carbon is examined in differen ganic compounds separated from the tissue, a the molecule into which the carbon dioxide been built is identified by its extra content of c bon of mass 13. Two curves showing the ions fr carbon dioxide are given in Figure 13. One these was enriched with the radioactive carbo mass 14.

Mass spectrometers of this type can detect presence of a very small amount of one gas e when it is mixed with large amounts of other ga Recently it has been found, for example, that m besides carbon dioxide is extracted by the lu from the blood and exhaled in the breath. The ma spectrometer illustrated in Figure 14 was b by A. O. Nier and A. Hustrulid in 1943 for dete ing leaks in vacuum equipment. Permanent ma nets are mounted inside the glass tube, and t instrument is adjusted to indicate the presence

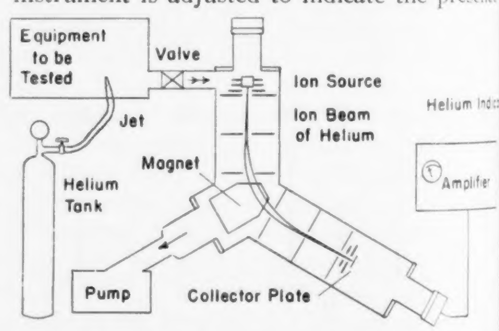


Fig. 14. Mass spectrometer for detecting leaks (Nier a Hustrulid, 1943).

lium. Helium gas from a small jet is allowed to ow over suspected points in the equipment, and soon as a hole is reached helium enters and mediately indicates the location of the leak.

Separation of isotopes. Every mass spectroaph separates isotopes, but usually only in the nount necessary to record their presence. In many ses, however, larger amounts have been collected. bout one fifth of a milligram of the 2 lithium otopes was collected by Oliphant, Shire, and rowther in 1934 for studying their transformaon under proton and deuteron bombardment. o determine which isotope of potassium emitted eta rays, 20 milligrams were separated in 1937 by . Smythe and A. Hemmendinger and collected in ollow containers. Similarly, to show that it was the bidium isotope at mass 87 that emitted beta rays, e same experimenters collected 2 milligrams of e separated isotopes. In 1944 separated isotopes thallium and silver were collected by W. Walcher r optical studies at the rate of 1.5 micrograms r hour. A. E. Shaw has collected the separated otopes of samarium at the rate of 2 micrograms r hour.

The most sensational of these small-scale separaons, however, was the separation in 1940 of 4 icrograms of uranium by Nier and 2 micrograms K. H. Kingdon and H. C. Pollock. With these mples the fundamental scientific discovery was ade and confirmed that the faint isotope of mass 35 was extremely susceptible to fission, and that reasonable amount of this isotope, if separated, ould constitute an explosive of unprecedented olence. One method that was developed to separte this isotope in large amounts used the magtic deflection method of mass spectroscopy; howver, the most important element in its success was e skill, based on cyclotron experience, with which the many engineering problems were met. The instrument called the "calutron" was adapted in 1946 to separating milligram amounts of the isotopes of many chemical elements. These are now available for experimental use in physics, biology, and metallurgy.

Other methods for separating the uranium isotopes were based on thermal diffusion or diffusion through a barrier. In the development of all these separation methods, a mass spectrometer was essential, in order to observe the enrichment actually attained. It was the compass that brought these great ventures safely to port. That these instruments could be rapidly provided when the need arose was possible only because of the experience gained in this country during the previous decade in the scientific investigations already referred to. No more striking demonstration of the importance of scientific "capital" for the national welfare could be imagined.

At the present time the subject of mass spectroscopy has two distinct aspects; on the one hand, it is still a fertile field of scientific investigation, and, on the other, it provides instruments, methods, and materials for investigations in chemistry, biology, and geology, and for the control of industrial processes. The study of the isotopic constitution of the elements, although almost completed for the stable isotopes, is still very active in the analysis of many of the radioactive isotopes; the natural abundances have still to be measured with the precision needed to detect variations brought about by many enrichment processes; and the measurement of masses is far from adequate, both in the accuracy attained and in the small fraction of the isotopes for which measurements of any kind exist. Because of the position of the subject in relation to theory and applications, we may confidently expect significant advances in the decades ahead.

GENES, CYTOPLASM, AND ENVIRONMENT IN PARAMECIUM*

TRACY M. SONNEBORN

In 1946 Professor Sonneborn, of the Department of Zoology, Indiana University, shared half the Annual Thousand Dollar AAAS Prize with Miss Ruth Dippell and Miss Winifred Jacobson, also of Indiana University.

AMONG organisms of all sorts, differences between individuals in hereditary traits are due to differences in nuclear genes. The remainder of the cell, the cytoplasm, is usually conceived as playing in most cases a passive role, of little or no significance in heredity. So far as environment is concerned, it may determine to what extent a hereditary trait will manifest itself, or indeed whether it will manifest itself at all, in a particular individual; but these environmental effects are not lasting: they do not extend to subsequent generations.

These generalizations must seemingly be reversed when one turns from the individual to the parts of an individual. The cells of the body of a higher organism are of many different kinds—muscle, nerve, connective tissue, and so on. Yet there is reason to believe that all these kinds of cells in one body have exactly the same set of genes. They arise by repeated divisions from a single cell, the egg, and at each cell division the nucleus with its genes seems to be partitioned with precise equality. How then can one explain the diversity among the body cells if the genes control their traits and if all have the same genes? The problem is even more puzzling. Different cell types from one body may be grown as long as desired in tissue cultures or in grafts, and many of the cell differences persist. They are, in effect, hereditary differences among cells apparently identical in genes. Although little is really known as to the explanation of these facts, it is suspected that cytoplasm and environment may be important in this sort of heredity, "heredity on the cellular level," which must play an essential part in the development of every individual from the egg.

One of the great bars blocking progress in exploration of these problems is the nasty fact that diverse body cells cannot be bred to each other the way Mendel bred peas and Morgan bred fruit flies. Yet such crossbreeding was the essential prerequisite for discovery of the mechanism of inheritance of the individual. As there seems at present no likelihood, at least in the foreseeable future, that body cells can be mated to each other, the outlook for analysis of the nature of the hereditary ferences among body cells is not promising.

There is available, however, an indirect second-best method of tackling these proble Among organisms that consist of but a single the group of individuals produced from a sin ancestral cell by repeated divisions is fundam tally comparable to the group of body cells prod from an egg. There is no doubt but that the of such a group of unicellular individuals all h the same genes; but unfortunately they also us have exactly the same hereditary traits. In s cases, however, they may develop different tr And, what is more important, *any* of these in certain species can be mated, like peas or flies. Here then is a great opportunity to tr find out how it happens that cells with the s genes can develop hereditarily different traits, to discover what parts are played in this by ge by cytoplasm, and by environment.

In the hope that such an investigation m throw light on these fundamental problems, w are pregnant with implications for an understa ing of normal and pathological developmen higher organisms, there has been in progress eighteen years in my laboratory an intensive s of the common and familiar unicellular an *Paramecium aurelia*. After twelve years of w which must be considered as having only prep the way for the main job, the past six years work have begun to lead to solutions of the p lems at the center of interest. The following count is an attempt to present briefly the results.*

Studies on heredity in paramecium go back i years to the pioneer researches of Jennings in period just before the fruit fly *Drosophila* bec the password to epoch-making discoveries ab the mechanism of heredity. For thirty ye

* In this recent work, there have been numerous collaborators: my research associates Ruth V. Di and Arlene LeSuer; research assistants Winifred Ja son, Betty Bartel, Jeanne Whallon, and Brunhilde M mert; technicians Marcella Chandler and Jewell S dill; graduate student John R. Preer, more recently independent investigator making important contribu in his own laboratory at the University of Pennsylva

However, the study of heredity in paramecium remained to a large extent outside the main currents of genetic advance, because of an important limitation in technical possibilities. Investigations by Maupas, the French librarian at Algiers, had shown by 1888 that, when paramecia come together in pairs and adhere, they fertilize each other and undergo essentially the same nuclear processes that take place in the mating of higher organisms. Yet this mating, or conjugation, could not very well be used in studies of heredity, because paramecia with different hereditary traits could not be made to conjugate with each other. As the crossbreeding of diverse hereditary types is the first requirement for the sort of study we wished to make, this type of work was not possible. In spite of this technical limitation, much of interest and significance was discovered, as a perusal of Jennings' review on *Genetics of the Protozoa* will show.

During the 1930s, determined efforts were made to overcome this technical limitation. These finally led to Sonneborn's discovery of sexes, or mating types, in paramecium. As the mating types themselves proved to be hereditary, it was easy to start with one animal and raise large cultures in which all the animals were of one mating type. When such a culture was added to a culture of the opposite mating type, the animals at once began to stick together and form large clumps. In an hour or so these clumps broke up into pairs of conjugating animals, each pair consisting of one animal of each mating type. With this knowledge, crossbreeding animals with different hereditary traits became as simple as in higher organisms; it was only necessary to mix together two cultures differing in mating type as well as in the other trait one wished to study, and the necessary mating would occur immediately.

When individuals of the single species *Paramecium aurelia* were collected from various parts of the United States, grown into stock cultures, and studied with respect to their mating types, it was discovered that this species is really a *group* of eight "physiological species," or "varieties," as they are called. Each variety has two mating types that interbreed freely, but interbreeding between different varieties is practically impossible.

After this foundation had been laid, progress in the study of heredity proceeded apace. The Mendelian laws were quickly found to operate in paramecium just as in the heredity of higher organisms; genes and chromosomes behaved at conjugation just as in the maturation and fertilization processes of higher organisms.

Two kinds of traits have been examined in a study of genes, cytoplasm, and environment in the control of heredity in paramecium. Surprisingly, two different systems of control are involved for these two kinds of traits. Yet the two systems are similar in some important respects, and each seems to have implications of broad and general significance. (A full account of the background material, together with much of what follows, will be found in a review by Sonneborn [1947].)

GENES, CYTOPLASM, AND ENVIRONMENT IN THE CONTROL OF THE "KILLER" TRAIT

In varieties 2 and 4 occur several stocks known as "killers" because fluid in which they have lived kills paramecia of other stocks. The latter are therefore called "sensitives." The killing substance in the fluid has been identified by van Wagtendonk as belonging to the class of chemicals known as desoxyribonucleoproteins. Such chemicals are of special interest, because they occur in chromosomes and viruses and are believed to be the stuff that genes are made of. This killing substance, which is known as "paramecin," also occurs inside the body of killer paramecia, but in them it does no harm. Paramecin is among the most active of biological substances: a sensitive paramecium can be killed by a single particle of it. Precisely what a single particle may be, however, is not entirely clear. Quite possibly a single particle is a single molecule, but it may be a highly polymerized molecule or even an aggregate of molecules. Each killer animal carries only a small number of particles of paramecin in its body. Samples of fluid containing many paramecin particles have little or no killing action on sensitive paramecia if they are conjugating, are at low temperatures, or have an abundance of food available.

There are a number of different kinds of paramecin, each produced by a different strain of killer paramecia. Two kinds have been studied very fully. One kind, produced by stock 51 of variety 4, acts so as to produce clear blisters on the surface of sensitive animals within a few hours. Later, the body form alters, a hump developing near the hind end of the body on the side away from the mouth.

visiting investigators in my laboratory, Professor Mary L. Austin, of Wellesley College, and Dr. G. H. Beale, of the Institute of Animal Genetics, Edinburgh; my colleague, the biochemist W. J. van Wagtendonk, and his research assistants, Patricia Hackett, Leonard Zill, and Donald Simonsen. For the privilege and means of carrying on cooperative investigations of this magnitude, we are deeply indebted to Indiana University for encouragement, financial support, working facilities, space, and that indispensable item—time for research; to the Rockefeller Foundation, the National Institute of Health, and the Jane Coffin Childs Memorial Fund for Medical Research, for liberal financial assistance.

The animals stop feeding, move slowly, grow smaller, round up, and finally die within 24–48 hours. Another kind of paramecin, produced by stock G of variety 2, acts more slowly. Early in the reaction, sensitive animals move aberrantly, often reversing the direction of their normal spiral movements and undergoing violent spinning on their longitudinal axis. They stop feeding, move more slowly, and die after several days. Still other kinds of paramecin act with great speed, killing their victims within a few hours.

The capacity to produce a particular kind of paramecin is inherited. This part of the work was carried out most fully on the killer stock 51 of variety 4 and on the sensitive stocks, 29, 32, and 47, of the same variety. Each of these stocks reproduces absolutely true to type under standard conditions. Within stock 51, all animals are killers; the trait is inherited through years of reproduction: thousands of cell divisions and many conjugations. With equal stability, all cultures of stocks 29, 32, and 47 are pure sensitives through fission and conjugation.

When the killer stock is mated to the sensitive stock 47, the breeding analysis shows that the two stocks do not differ in any gene affecting the killer or sensitive traits, but that this difference in traits is due to a difference in cytoplasm. The production of paramecin depends on a cytoplasmic factor, called "kappa;" when this gets into sensitive animals from stock 47, it converts them into killers. All the progeny of the converted animals are also killers and contain kappa, so it must multiply. It thus has the property of self-multiplication, the basic property of genes. Miss Dippell has shown that kappa also mutates like a gene, mutant kappas controlling the production of different kinds of paramecin which kill sensitive animals in different ways, produce paramecin more slowly, or differ in still other ways. Nevertheless, kappa is not in the nucleus and is therefore not a gene in the usual sense.

On the other hand, neither is kappa entirely independent of the genes. This was discovered by breeding analyses following the mating of the killer stock 51 to either of the sensitive stocks 29 or 32. The results showed that a dominant gene *K* must be present if kappa is to persist and multiply; when an alternative recessive gene *k* is present in pure (homozygous) form, kappa cannot survive. Stock 51 is pure for gene *K*, stocks 29 and 32 are pure for gene *k*. When stock 29 or 32 is bred so as to replace their *k* gene by *K*, they remain sensitive and no kappa arises; hence the gene *K* cannot make kappa arise. But if some kappa is also added when *K* has been introduced, then the kappa multiplies and transforms the animals into killers. Conversely, when animals of the killer stock have their *K* gene replaced by *k*, kappa disappears and they transform into hereditary sensitives. As expected, restoration of the *K* gene cannot restore the killer trait: kappa is irretrievably gone.

The gene *K* thus controls something in the organism that is essential for the maintenance of kappa, but it cannot initiate the production of kappa if none is present to start with. Breeding analysis showed that stock 47 contains the gene *K* but lacks kappa. That is why, in the crosses to the killer stock, inheritance seemed purely cytoplasmic with no gene control at all. The gene control was there, but it could not be discovered because the two stocks (51 and 47) were alike in having the required gene.

Much more insight into the nature of kappa was obtained by a study of the effect of environmental conditions on it. The kappa in stock G is a slowly multiplying kappa. When the animals are given an excess of food, they multiply more rapidly than kappa; consequently, the amount of kappa per cell gradually declines. By varying the growth rate of the paramecia, Preer found that they could maintain kappa only if grown no more rapidly than two divisions per day, which is therefore the maximum rate at which kappa can multiply in this stock. When the animals were grown more rapidly and the average concentration of kappa per cell steadily declined, the animals transformed, first to resistant nonkillers, then to sensitives, which could be converted back to killers by growing them slowly, and finally to irreversible sensitives that had lost kappa. Using data obtained from ingenious experiments, including some in which kappa was destroyed by X-rays, Preer calculated that there were approximately a few hundred kappa particles in the original killer animals. Mathematical methods, essential for this sort of analysis, were developed for our use by Richard Otter, of Princeton University. Similar estimates of particle number for the stock 51 kappa were obtained by others, using high temperatures to stop kappa multiplication or destroy it.

As the preceding results prove, kappa, though genelike, is both gene-dependent and environment-dependent. A hereditary killer can be transformed into a hereditary sensitive either by substituting the gene *k* for its *K* gene or by growing the animals rapidly, subjecting them to X-rays, or exposing them to high temperatures. Moreover, these environmental effects—if carried to the point where *all* kappa is gone—are irreversible; they are permanently inherited. Preer showed that if a single

particle of kappa remains, it will, under conditions of slow growth of the animals, restore the full original amount and result in reversion to the killer trait.

In view of some of these results, the question has been raised by Altenburg, Lindegren, and Spiegelman as to whether kappa might be a symbiotic organism. Two recent discoveries are of interest in that connection. First, Sonneborn discovered that kappa is, under extreme conditions, "infectious." When sensitive animals with the *K* gene are exposed to immense concentrations of kappa in their culture fluid, a certain fraction of them acquire kappa and transform into permanent hereditary killers. This apparently never happens except when kappa is concentrated enormously beyond the limits ever likely to occur in nature. Second, Preer found that kappa is much larger than a gene; it is in fact larger than most viruses, as large as rickettsias or small bacteria. This remarkable discovery was made by following two clues. The X-ray inactivation studies indicated a particle of about this size. The discovery of the desoxyribonucleoprotein nature of paramecin suggested that kappa also might be of similar nature. As there is a specific staining method (the Feulgen method) for such substances, Preer used this stain and found particles of the expected size in killer, but none in sensitive, paramecia.

This brings the status of investigation of kappa up to date. There is still no decisive evidence on which to base a secure judgment as to whether kappa is a symbiotic organism. Essentially, the same uncertainty exists for a comparable cytoplasmic factor in the fruit fly *Drosophila* investigated by L'Heritier and his collaborators. So far as paramecium is concerned, however, it may be said that the staining methods that reveal kappa do not reveal other particles of similar size and chemical nature. Kappa may therefore be an example of a rare and exceptional group of phenomena. Its study, however, emphasized the difficulty of distinguishing between a normal cytoplasmic genetic factor and a symbiotic or parasitic microorganism. Any pathogenic or symbiotic organism that became established in the cytoplasm and passed from generation to generation through the cytoplasm of the egg could simulate the behavior of kappa. There is, in fact, reason to suspect that comparable agents are involved in the causation of certain types of cancer. It has further been suggested by Darlington that these agents may not be organisms, but self-multiplying particles or plasmagenes native and normal to the cytoplasm of other creatures than those in which they cause disease. Whatever may be the correct interpretation of kappa, it is remarkable that its pathogenic action is confined, not to those animals in which it occurs, but to those that lack it. It controls the formation of paramecin, which is harmless to animals containing a full allotment of kappa, but lethal to those paramecia that have no kappa or too little of it. Perhaps further work will throw light on the knotty problem of the origin and nature of kappa and the comparable events involved in mammary cancer of mice, sarcoma of fowl, and resistance to CO_2 in the fruit fly.

GENES, CYTOPLASM, AND ENVIRONMENT IN THE CONTROL OF ANTIGENIC TRAITS

Within the past year, attention has been directed to other traits in variety 4 of *P. aurelia* which show an even more interesting and probably more general system of interrelations between genes, cytoplasm, and environment. When paramecia are injected into a rabbit several times, blood serum obtained from the rabbit, even when greatly diluted with water, will paralyze the normally rapid-swimming paramecia. This paralytic action of the serum is due to union of specific chemicals in the paramecia, called antigens, with corresponding specific chemicals in the serum, called antibodies. The antibodies, are formed in the rabbit's blood in response to antigens of the injected paramecia. This is a highly specific reaction. If two different strains of paramecia, *A* and *B*, are injected into different rabbits, the serum obtained from the rabbit injected with *A* paramecia will paralyze paramecia of strain *A* but may have no effect on the *B* paramecia, and vice versa.

Six such diverse strains (*A, B, C, D, E,* and *G*) arose within the one stock 51 of variety 4, and six specific antisera were obtained against them. The six diluted antisera each paralyze specifically only animals of the corresponding strain or antigenic type. By serological methods, it was shown that the paralysis reaction on each type is due to interaction between *one* kind of antigen and its corresponding antibodies. There are thus six different antigens in the six types of strains. Each of these six types is strictly inherited under standard conditions of culture (27° C with enough food to permit one fission per day).

When these six types are mated to one another and the hybrids are analyzed by further breeding, the results show that the differences in antigenic type depend not on differences in genes, but on cytoplasmic differences. The cytoplasmic factors involved here, however, cannot be seen by the staining method used for demonstrating kappa.

As with kappa, the cytoplasmic factors, or plasmagenes, involved in the control of these antigens are subject to environmental influence. When animals of any one of these six types are placed in a few drops of the corresponding paralyzing antiserum, all the animals are paralyzed; but if the serum is dilute enough, all will recover when removed from the serum. The recovered animals may be isolated and each will multiply to form as large a culture as desired. Surprisingly, when such cultures are now subjected to the same antiserum again, they are no longer paralyzed by it. Instead, they are now paralyzed by a different one of the six antisera. In other words, they have been transformed to a different one of the six antigenic types. Moreover, this transformation is inherited thereafter as long as the cultures are grown under standard conditions. Large proportions of the exposed animals are hereditarily transformed, up to 100 percent in some cases. In an analysis of the details of one of these transformations, Beale has found that the original antigen ceases to multiply after exposure to antiserum and another antigen begins to appear after several hours. The latter gradually increases until transformation is complete. This suggests that the antigen is itself a plasmagene which cannot multiply when combined with antibody. This gives an opportunity for another plasmagene to multiply and replace it.

These facts about transformation of antigenic type imply that each animal actually has several possibilities, most of which are not or cannot be realized without sacrificing other possibilities. There is clearly something mutually exclusive about these possibilities, for an animal can manifest only *one* of the six antigens at a time. By the method of exposure to paralyzing antiserum, it can be transformed (directly or indirectly) so as to manifest *any* one of the six antigens. Moreover, the transformations with antiserum involve no irreversible loss of capacities, because further exposure of transformed animals to antisera which can paralyze *them* can bring the organisms back to their previous antigenic type. Hence, the capacity to produce all six antigens is inherent in all animals of stock 51, but only one—any one—of these six capacities can be realized at any one time. Since the antigenic type has been shown to be determined by cytoplasmic factors, or plasmagenes, and since the mechanism of transformation of type with antiserum indicates that the antigens are themselves the plasmagenes, there must be a different kind of plasmagene for each of the six kinds of antigens. The existence of different plasmagenes for different antigens is further neatly shown by the discriminating action of each of the six antisera: each suppresses one, only one, and always a different one of the six antigen plasmagenes.

As already indicated, transformation of antigenic type involves great increase of an antigen of which little or none could previously be detected. It might be supposed that change of antigenic type could involve only a shift of position, without change in quantity, of the antigens. Antibodies are very large molecules, and it seems improbable that they penetrate the cell; more likely, they act only on the surface of the cells. Hence, they could unite only with antigens on the cell surface. If antigens inside the cell wandered to the surface, this could conceivably be a mechanism of transformation of antigenic type without resorting to an increase of the new antigen at all. This possibility seems unlikely, however, in view of the serological evidence. The paramecia are thoroughly broken up before they are injected into the rabbits to induce antibody formation. Antibodies against more than one of the six antigens are sometimes found in the antiserum, but the great bulk of the antibodies present are specific for the one antigen that characterizes the type of the injected paramecia. The same conclusion is indicated by the reverse experiment, for it is possible to discover what antigens are present in animals by their capacity to inactivate a serum: the antigens present combine with their corresponding antibodies in the serum and remove them from solution. In this way it can be shown, using either whole or broken-up paramecia, that each antigenic type contains mainly or exclusively one type of antigen, only traces of the other antigens being present if they are present at all. Hence, change of antigenic type must involve an enormous increase in the antigen characteristic of the type to which an organism transforms.

This leaves an interesting problem. An animal ready to transform as a result of exposure to paralyzing antiserum has five possibilities open to it. What determines which one of these five other antigens will multiply and replace the antigen inactivated by antibody? On the hypothesis that competition is involved here, with the best-adapted antigen taking over, attempts were made to discover how to control which antigen would take over. In view of the previously proved importance of temperature and food supply on the growth of kappa, and a priori probability that these conditions, if any, might be important in influencing the outcome of competition among plasmagenes, experiments to test their action were carried out. They proved to be decisive.

Animals exposed to paralyzing antiserum trans

form to one type at high temperature, to a different type at low temperature; to one type when fed abundantly, to another type when fed little. With this clue, the same conditions of temperature and food supply were employed *without exposing the animals to paralyzing antibodies.* These alone also brought about transformations. But now the transformations occurred much more slowly: with antiserum, transformation is complete within 2 or 3 fissions; without antiserum, 20, 30, or more fissions are required to transform. This difference in rate of transformation was actually predicted because antiserum had been interpreted as "knocking out" the main competitor antigen plasmagene, a process that should greatly accelerate transformation. Regardless of how the transformations are brought about, they are permanently inherited, even through sexual reproduction, under standard conditions of culture.

These results seem to throw light on some remarkable, long-known, but puzzling facts in parasitology. Another kind of unicellular animal, the trypanosome, may undergo similar transformations in the course of its infection of the mammalian host. Some trypanosomes cause sleeping sickness and other diseases in man. In laboratory animals, such as the dog, after the trypanosomes have greatly multiplied in the blood and the dog is very sick, there comes a crisis and the dog seems to recover. Trypanosomes practically disappear from the blood, because of the action of induced antibodies formed against the trypanosomes. But a relapse occurs and trypanosomes again teem in the blood; they had not all been killed off. The new trypanosome population is unaffected by the antibodies that had been built up in the dog, for the parasites are of a different antigenic type! Now comes a new crisis, another depletion of the trypanosome population, due to the action of new antibodies formed against the new type of trypanosome. But this is far from the end. Relapse and crisis may alternate a dozen times or more before the dog or the trypanosome finally gives in and dies. With each relapse, the trypanosomes have a new antigenic type; with each crisis, the dog has built up a new kind of antibody.

As with the transformations of antigenic type in paramecia, the diverse types of trypanosomes reproduce true to type. This is shown by cultivating them in an organism like the mouse that fails to form antibodies against them. In the case of the trypanosomes (as in other, similar cases in other organisms), it has long been supposed that the antiserum acted as a selective agent, killing off all the trypanosomes except a few that had spontaneously (and independently of exposure to antibodies) mutated to a new antigenic type. The results with paramecium show that the same kind of result can occur when selection of spontaneous mutations is excluded; that the "mutants" are indeed stimulated to arise by the action of the antibodies; and that the "mutants" are not mutants in the sense of having changed genes or nuclei, but differ only in their cytoplasm, one antigen plasmagene rising in concentration while another declines.

Thus far only the roles of cytoplasm and environment in the control of antigenic type in the paramecia have been set forth. All the types are alike in genes, and so the genes seem to have no determining effect at all. Further analysis shows, on the contrary, that this is an incomplete and false picture. The role of genes was discovered by extending the studies to stock 29, another stock of variety 4. In this stock, six antigenic types have also been found. The results obtained with the six antigenic types of stock 51 have all been confirmed with the six types in stock 29. But the antigenic types occurring in these two stocks are not identical. In stock 29, four of the six types are similar to the types *A, B, C,* and *D* in stock 51, though they are not exactly the same; the other two types, *F* and *H,* do not occur in stock 51; and the two types *E* and *G* of stock 51 do not occur in stock 29.

When one performs breeding experiments to determine the basis of the difference between the two stocks in the capacity to form antigen *F,* it turns out that this is due to a difference in a single pair of genes. The capacity to make the *F* antigen plasmagene depends on a gene which is present in stock 29, absent in stock 51. Note that the possession of this gene does not assure that the possessor *will* be type *F;* it merely means the possessor *can* be type *F.* As the methods of transforming any type to type *F* (if the required gene is present) have been discovered, it is always possible to determine when the gene is present.

From these results, it looks as if each of the antigen plasmagenes can be formed when the proper genes are present, not otherwise. In this respect, the results are very different from the results with kappa and the killer trait, for kappa could not be formed under the action of the required genes unless some kappa was already present. As the overwhelming importance of genes in the control of hereditary traits is abundantly demonstrated by investigations on all sorts of organisms during the present century, the system of determination and inheritance manifested by the antigenic traits seems likely to be more general and

significant than the system earlier discovered for the killer trait.

WE RETURN now to the problem with which we started. What may our discoveries signify with reference to the dark problem of how the cells of complex higher organisms come to be diverse? Like the different antigenic types within one stock of paramecia, the different cell types in the body of a higher organism probably have the same genes. The results on paramecium show how cells with the same genes can differ persistently in their traits as a result of competition between alternative and mutually exclusive plasmagenes, in spite of the fact that the plasmagenes themselves are dependent upon genes for their origin and maintenance. There is at least a possibility that similar mechanisms operate in the differentiation of different cell types in the body of a higher organism. If so, there emerges the hope of similar control of cell type, of control of the process of transformation from one cell type to another, including even the transformation from normal to cancer cells and, what is of greater practical importance, the reverse transformation.

This account of the status of the work on antigenic traits in paramecium gives it perhaps too great an aspect of finality. It has now been in progress only a year and is therefore properly to be considered as in its early stages. What will emerge as the study progresses cannot of course be predicted, but there is reason to hope that whatever emerges must be of interest in relation to problems of cellular heredity, differentiation, and transformation, wherever they are met in both unicellular and higher organisms.

SYMPHONY AT SEA

From where I sit and see—
The sheets with reef points more like notes,
A vane which marks the mood
Of a maestro's wand—
I hear a symphony.
The gusty thumb and digits strum
The strings, the halyards and the stays.
The wand waves to and fro, fortissimo;
The mood becomes allegro
And crashing tympanists are wet
With their percussion.
On high a throated chorus
Dances in the wind
Like smoke caught in a draft.
I think the music will not end
When someone stands and shouts,
(It might be "bravo")
As clouds uncurtain jewelings on the land.
The notes die with the fluttering sheets
And lapping waves applaud politely
On the hull in port.

JOSEPH HIRSH

NUCLEAR PHYSICS AND HIGH-VOLTAGE ACCELERATORS

MERLE A. TUVE, LAWRENCE R. HAFSTAD, and ODD DAHL

Drs. Tuve, Hafstad, and Dahl, all of the Carnegie Institution of Washington, collaborated on the paper that was awarded the Annual Thousand Dollar AAAS Prize in 1930.

IN OUR paper at Cleveland (Experiments with High-Voltage Tubes, *Phys. Rev.*, 1931, **37**, 469) in December 1930, which received the AAAS prize for that year, we reported on the first production of artificially accelerated particles (electrons in the beta-ray region) and radiations having energies above one million volts (electrons in the beta-ray region and X-rays in the gamma-ray region). Lacking a better voltage source at that time, we had used a Tesla coil for the development and testing of cascade-type (multiple electrode) tubes for voltages above one million, and to carry out these measurements on high-energy radiations and particles. We also reported then the first observations on animals of whole-body lethal and sublethal exposures to gamma rays, which have been of increasing interest in recent years. We had studied the effect of large dosages of highly filtered gamma rays on rats (using 6 grams of radium at the Bureau of Standards).

Viewing the enormous progress in nuclear physics using high-energy accelerators since 1931, we feel that it is highly immodest to list even the high lights of "what has happened in the field of our paper" since we gave it. We were early workers, beginning in 1927, in a field that was enormously rich in immediate potentialities, even beyond our own recognition and great expectations. The cyclotron (1932); the first artificial transmutations (1932); the discovery of the neutron and positron and their artificial production (1932–34); the host of nuclear transmutations produced by high-energy particle accelerators (1932–39 and to date); the measurement of the primary nuclear forces (1935–37); the discovery of resonance reactions and studies of nuclear energy levels (1935–37 and to date); the work on nuclear fission, after its first glimpse using radium sources; even the atomic pile and its consequences—all were more or less direct consequences of the burgeoning enthusiasm of the early workers with high-voltage sources (1926–32), who had the clear-cut aim of producing artificial beams of high-energy particles for the direct study of the atomic nucleus. We were privileged to be among this group, but our early efforts, reported in the 1930 paper, contributed only in a very limited and modest way to the flowering of knowledge and activity in this area which began about 1932.

Our own activities during the decade of the thirties were largely focused on our original 1926 goal of studying "the simplest interactions between the primary particles at very close distances." This resulted in our measurements of the proton-proton interaction, which turned out to be very nearly the same as the proton-neutron interaction, not even visualized when we began. These interactions are the binding forces of atomic nuclei, and may ultimately be further explained in terms of mesons. We also made early studies of nuclear resonance reactions (Hafstad, L. R., and Tuve, M. A. Resonance Transmutations by Protons. *Phys. Rev.*, 1935, **47**, 506–7) and other transmutations of the light elements, and participated with our colleagues in early studies of nuclear fission and the emission of delayed neutrons (Roberts, R. B., Hafstad, L. R., Meyer, R. C., and Wang, P. *Phys. Rev.*, 1939, **55**, 664).

We like to think that our enthusiasm and that of our colleagues, especially Gregory Breit, for the possibilities of discovery and the basic value of new ideas and facts of philosophical importance in this area may have contributed in a characteristically infectious way during the early period to the initiative and vigor of some of the other workers who chose to enter the field of nuclear physics using artificially accelerated particles, but the development of this entire field since our paper of 1930 has been so vast that we cannot meet the request of the editors of this journal for a résumé of later events in the field of our paper, and must disclaim any detectable vein of influence on later developments which can be attributed to our 1930 report. We greatly enjoyed participating, however, in the rapid development of modern nuclear physics, indubitably based on the leadership of that great and humble man from New Zealand, Lord Rutherford.

THE TISSUES IN INFECTION AND IMMUNITY

REUBEN L. KAHN

Professor Kahn, University Hospital, University of Michigan, received the Annual Thousand Dollar AAAS Prize in 1933.

IF YOU have had a sore throat lately, you were probably greatly annoyed by the discomfort the infection caused you. Actually, what you experienced was an outstanding defensive maneuver of the body against infectious microorganisms. Virulent streptococci caught you off guard—you exposed yourself to them in a state of low resistance, thus enabling them to gain a foothold in your throat. Even with their initial advantage over you, your body was able to imprison them in the very area of their attack and hold them there until reinforcements, especially fluid, phagocytes, and antibodies were brought by the circulation in sufficient quantity to destroy them.

Had these microorganisms, localized in the circumscribed area in the throat, had the opportunity of breaking through the prison bars and entering the blood stream, they would have gained a vital advantage in their warfare with you. Like an enemy gaining access to the transportation facilities of a country, the microorganisms could then have established numerous foci of infection in different parts of the body. But the chances are against such an occurrence. Once in a long while large numbers of microorganisms of exceptionally marked virulence attack a host of unusually low resistance. Then the capacity of the body to keep the microorganisms confined to the area of attack may be reduced, giving the enemy the upper hand. Under ordinary conditions, however, the localizing capacity of the tissues is sufficiently powerful to keep the microorganisms confined to the area of attack.

Our ability to keep the streptococci localized in the case of a sore throat is intimately bound up with our natural immunity to these microorganisms—the result of our close contact with streptococci through the ages, in which we have been subjected to innumerable subclinical attacks by them, similar to vaccinations with minute doses of these microorganisms. These attacks have in turn called forth defensive responses, which gradually built up in us a considerable degree of natural immunity, unquestionably saving us from streptococcic infections on many occasions when our surface tissues, particularly the skin and exposed mucous membranes, come in contact with these microorganisms. When, however, owing to our lowered resista or to the high virulence of the microorganisms to both, they succeed in gaining a foothold in so exposed tissue of the body, the localizing pheno non immediately manifests itself.

The localization of attacking microorganisms our tissues in the immune state is apparently a versal law in parasitism, for we see this locali tion not only in human beings and in animals, also in plants. This localization, also referred t "retention" or "fixation," gives the microorgan a chance to establish themselves in a tissue o host, and it aids the host by keeping the mic organisms restricted to a local area. Whether parasite remains chronically localized or is rap destroyed in the area depends on many fact such as the virulence of the parasite, the number parasites taking part in the attack, the resistance the host, and the tissue involved.

The localizing phenomenon is not limited living microorganisms. The tissues of an imm host manifest this phenomenon also against n living microorganisms. That dead microorganis can replace living microorganisms in calling fo immunity was a revolutionary observation made this country by Salmon and Smith in 1886. Paste had just shown that attenuated microorganis instead of virulent ones, could be used in immuni ing chickens against chicken cholera. But t assumption was still prevalent that microorganis must be alive in order to produce immunity. T observation of Salmon and Smith placed immun reactions in the realm of chemical or colloid-che ical reactions. If dead microorganisms can serve antigens in immunizing and in testing the extent immunity in animals, it means that chemical co stituents of the microorganisms, such as prote and other fractions, play a major role in immun

The localizing phenomenon is also manifest against antigenic substances unrelated to micr organisms, such as proteins from any source. As matter of fact, egg and serum albumin, or prot mixtures, such as horse serum, are common an gens employed in immunity studies, both in t production of antibodies in animals and in calli forth tissue reactions. It would appear that

animal reacts to proteins and other antigens by the same defensive measures it utilizes in reacting to microorganisms.

Returning to infection in man, it is easy to understand why the capacity of the tissues in the immune host to localize attacking microorganisms is a vital defensive reaction. For, if our tissues lacked this capacity, every attacking microorganism would enter the blood stream and become widespread throughout the body. This localizing tendency persists even until death. For example, a person with tuberculosis may have but a single tubercle in the lung; or perhaps an entire lobe of a lung may be involved; or, indeed, in rare instances, an entire lung. Yet the localization of the tubercle bacilli is so powerful that the remaining tissues of the infected person might be free from tubercles. Even in miliary tuberculosis, when the tubercle bacilli have become widespread throughout the body and there is little hope for recovery, we see innumerable small, circumscribed tubercles in different tissues of the host. The tubercles are ill-formed, to be sure. They are not like the thick-walled tubercles one sees under ordinary conditions in tuberculosis, but they represent a final attempt, so to speak, on the part of the tissues to localize and wall off the invaders.

Some eighteen years ago, I became interested in this localizing, or retention, capacity of the tissues for infectious microorganisms, proteins, and other antigens. In due time I devised an experiment to determine quantitatively the localizing capacities for protein antigen of different tissues in animals in the living state. It was my hope that the experimental findings of the tissue response to protein as an antigen would enlarge our knowledge of the tissue response to microorganisms in infection and immunity. Before considering that experiment, let us briefly examine the accepted views of the role of antibodies, phagocytes, and of the tissues in immunity.

LITTLE controversy exists now with regard to the functions of antibodies and phagocytes in immunity. Sixty years ago, in 1888, Nuttall, for many years professor of biology at Cambridge University, observed that fresh blood serum possessed some germicidal powers. This discovery may be looked upon as the forerunner of our knowledge of antibodies in immunity. One of the most helpful antibodies in immunity is diptheria antitoxin. This antibody was discovered by Roux and Yersin, also in 1888, at the Pasteur Institute, by growing diphtheria bacilli in broth and injecting animals with the broth filtrates. These filtrates contained diphtheria toxin and when injected in animals called forth the production of diphtheria antitoxin. Soon Behring and Kitasato showed that diphtheria antitoxin can be employed in human beings with diphtheria; that the antitoxin neutralizes the toxin in vivo.

Most microorganisms, however, exert their harmful effects not by the production of soluble toxin, like diphtheria or tetanus toxin, but by growing in the tissues and thereby interfering with normal cell life. Many types of streptococci and staphylococci, the typhoid bacilli, and the tubercle bacilli might be mentioned as illustrations of microorganisms in this category. These and many other microorganisms in this class are known to also call forth the production of antibodies. These antibodies, furthermore, are believed to be defensive in nature, although their mode of action is not as clear-cut as that of antitoxic antibodies. The same holds true of antibodies produced in animals by the injection of protein antigens.

Turning from antibodies to phagocytes, or white corpuscles, it was Metchnikoff, the Russian scientist, who first expounded their role in immunity. Metchnikoff was a zoologist, and his interest was not limited to the white corpuscles of mammals. He was interested also in the ways a unicellular animal like the *Amoeba* handles bacteria and foreign matter. Actually, the *Amoeba* digests bacteria by intracellular digestive enzymes, and foreign particles which it cannot digest, it extrudes from its monocellular body. In mammals, Metchnikoff observed that white corpuscles have the property of drawing bacteria into their bodies and destroying them, presumably also by digestive processes.

Metchnikoff's view that the white corpuscles are the soldier cells of the body was at first severely criticized by his colleagues, but ultimately it became the forerunner of the theory of phagocytosis. This phenomenon has come to embrace not only the role of white corpuscles, which are mobile and can rapidly accumulate in any area in the body where microorganisms open attack, but also the role of fixed phagocytes known as the reticulo-endothelial system, which includes numerous specialized cells, especially in the spleen and liver, which exert phagocytic powers not only against microorganisms but also against foreign particles. Phagocytosis is referred to as the "cellular" concept of immunity in contradistinction to the role of antibodies in immunity, which is referred to as the "humoral" concept.

Half a century ago, there was high controversy between these two concepts of immunity. This controversy was not free from international implica-

tions. The French school of workers in immunity, under the leadership of Metchnikoff at the Pasteur Institute, promulgated the view that phagocytes carry the burden of defense. The German school, under the leadership of Ehrlich, of Salvarsan fame, who fathered the side-chain theory of antigen-antibody reactions, insisted that antibodies carry the burden of defense. It was during this controversy that Metchnikoff made the famous remark, "Any German who does not accept Ehrlich's side-chain theory is damned."

Little is heard of that controversy among present-day immunologists. It is now generally accepted that both the antibodies and the phagocytic cells play important parts in protecting us against bacterial invasion and in defending us when microorganisms succeed in gaining a foothold in the body. When it comes to the function of the tissues in immunity, controversy again runs high. Textbooks of bacteriology and immunology do not generally even discuss tissue immunity; they present chapters only on tissue hypersensitiveness. As is well known, the defensive role of the fluids of the body is covered in these textbooks by chapters on "Antibodies" or on "Humoral Immunity;" the defensive role of the phagocytes, by chapters on "Phagocytosis;" and the role of the tissues in immunity, by chapters on "Tissue Hypersensitiveness."

The concept of tissue hypersensitiveness had its origin in "anaphylaxis," a term coined by Charles Richet, who was professor of physiology in the Medical Faculty of Paris, in 1902. The term anaphylaxis, "against protection," as distinguished from prophylaxis, or "for protection," was first employed in an article by Richet and Portier entitled "The Anaphylactic Action of Certain Poisons," which appeared on February 15, 1902, in the *Bulletin of the French Biological Society*. Richet and Portier reported that repeated injections in dogs of a glycerin extract of the tentacles of Actiniaria, which is poisonous, causes the animals to become sensitized instead of immunized to the poison. The accepted view had been that repeated injections of an injurious substance tended to produce tolerance or immunity to the substance. But here was evidence that repeated injections of a poison produced a condition of hypersensitivity.

Anaphylaxis has been investigated especially in guinea pigs. Theobald Smith in 1904 reported that guinea pigs often died suddenly after a second injection of horse serum given directly into the blood stream, and this experimental occurrence was for a number of years referred to as the "Theobald Smith Phenomenon." The death of the guinea pigs was due to asphyxia resulting from the acute spasm of the muscular coat, and the consequent valvelike closure, of the smaller bronchi. The important fact, however, was that the animal died with great suddenness following a second injection of the antigen in the blood stream.

One year after the report on anaphylaxis, M. Maurice Arthus, in an article entitled "Repeated Injections of Horse Serum in the Rabbit" (*Compt. Rendus Soc. de Biol.*, 1903, 55, 817) claimed to have corroborated the findings of Richet and Portier. This author observed that a first injection of horse serum in the rabbit, given by any route, has no harmful effect on the animal. But repeated injections, if given under the skin, produce local inflammatory reactions, with tissue necrosis. Arthus interpreted these reactions to be manifestations of local anaphylaxis.

These experiments by Richet and Arthus attracted a great deal of attention, and gradually the belief became prevalent that the tissues in infection and immunity apparently do not act in harmony with antibodies and phagocytes in defense of the host but that they become hypersensitive. To correlate tissue hypersensitiveness with humoral and cellular immunity in an infection, such as a sore throat, is to assume that antibodies and phagocytes are lined up against the streptococci in behalf of the host, whereas the tissue cells under attack in the throat are in a state of "against defense" and are lined up against the host in behalf of the streptococci.

Because it did not seem possible that the body under attack by microorganisms is divided against itself, I was led to undertake studies of the meaning of tissue hypersensitiveness. My first series of articles was published under the general heading of "Studies on Sensitization." In due time, these studies began to indicate that the tissues react in harmony with antibodies and phagocytes in defense of the host, and I changed the general heading of the articles to "Tissue Reactions in Immunity." Finally, I felt so confident of my premise of the defensive nature of the tissues in infection and immunity that I entitled my summation of these studies "Tissue Immunity" and not "Tissue Hypersensitiveness."

Our findings indicate that the sudden death of the guinea pig following a second injection of antigen in the blood stream, as well as the local inflammatory reaction in the skin of the rabbit to repeated injections, does not justify an assumption that the tissues in infection and immunity under natural conditions are in a state of hypersensitivity. They indeed indicate that the tissues, the antibodies, and the phagocytes work in harmony in defense of the

host. This view at once leads us into depths of controversy. Some authors accept the classical interpretation of tissue hypersensitiveness according to Richet and Arthus. Some maintain that hypersensitiveness is a phase of immunity and that it makes little difference what terminology one uses. Others look upon hypersensitiveness and immunity as two distinct coexisting phases in infections, such as in tuberculosis. Then, too, allergy in man, so intimately tied in with tissue hypersensitiveness, needs to be considered. It was in 1906 that the term "allergy" was created by von Pirquet in Vienna. Originally, it was intended to mean altered tissue reactivity, but the term has become a synonym for tissue hypersensitiveness.

With regard to the experiments to be described, no claim is made that they will give all the answers to the question of the role of the tissues in infection and immunity. Available knowledge of immunity is altogether too limited to give adequate answers to any comprehensive questions in this field. The claim is made merely that any concept that attempts to explain anaphylactic shock obtained under special conditions of injection actually can be said to apply only to parallel experiments carried out under the same conditions of injection. Since there is no record of any guinea pig or any other animal dying from anaphylaxis under natural conditions, any assumption that the tissues under natural conditions of infection and immunity are in an anaphylactic or hypersensitive state is beyond experimental indication.

Little need be said about the explanation of Arthus that the local inflammatory reaction in the skin of the rabbit to repeated injections of antigen is a local anaphylactic reaction. Many workers in immunity look upon this skin reaction as a defensive reaction, although some hold onto Arthus' concept that the rabbit, like the guinea pig, is in a state of tissue hypersensitivity as a result of the repeated injections.

Let us now turn to the experiments carried out in my laboratory on the localizing capacity of the tissues in immunity.

As ALREADY emphasized, when an animal possesses immunity to microorganisms, the tissues possess the capability of localizing them in the area in which they gained entry. Similarly, when an animal is immunized to a protein antigen, it will localize this antigen in the area of injection. The localizing mechanism developed against living microorganisms is apparently applied to protein and other antigens.

An experiment in which the localizing capacity of the skin of immunized animals is determined quantitatively employs a method, developed in my laboratory, which enables us to measure this capacity in terms of localizing, or retention, units. The localizing capacity of the skin of nonimmunized controls is then determined for comparison.

In this experiment, rabbits are immunized with horse serum. However, the use of horse serum in the quantitative determination of the localizing capacity of the skin of these animals would prove inadequate. Instead, therefore, a type of horse serum is employed which is quantitatively standardized on a unit basis. Actually, horse serum diphtheria antitoxin, standardized on the basis of neutralizing units of diphtheria toxin, is employed in testing the skin's localizing capacity.

The injection of antitoxin in the skin of these animals and the determination of the extent to which it is localized in the area of injection necessitate that they be injected also with a given dose of diphtheria toxin. Let us suppose that the antitoxin is injected in a given area in the skin of a rabbit that had been previously immunized with horse serum, and that diphtheria toxin is injected in another area. The skin will localize the injected antitoxin and prevent it from reaching the blood stream. That will mean that the diphtheria toxin will reach the blood stream and exert its toxic action unhampered. Suppose, however, the antitoxin is injected into the skin of a nonimmunized rabbit under the same experimental conditions. The antitoxin will reach the blood stream rapidly, neutralize the injected toxin, and thus prevent toxic action.

In an actual experiment, rabbits *A* and *B* are immunized to horse serum. Rabbits *C* and *D* are nonimmunized controls. All four rabbits are injected with horse serum antitoxin in the skin and, simultaneously, with a lethal dose of diphtheria toxin. The skin of the nonimmunized rabbits, having little capacity for localizing the injected antitoxin, will permit it to reach the blood stream without delay and thereby neutralize the diphtheria toxin. The skin of the horse serum-immunized rabbits, having a marked capacity for localizing the horse serum antitoxin, will prevent it from entering the blood stream and neutralizing the toxin. The nonimmunized control rabbits will thus survive, and the rabbits immunized to horse serum will succumb to the diphtheria toxin.

The following results were obtained under experimental conditions in which rabbits *A* and *B* were given two immunizing injections of horse serum, *C* and *D* no injections of horse serum, and all rabbits were given the same dose of diphtheria toxin (50 minimal lethal doses, 250-gr guinea pig).

Rabbit *A* was not protected from toxin death by 1,000 units of antitoxin injected into the skin, indicating that this amount of antitoxin was localized in the skin and was thereby prevented from reaching the blood stream and neutralizing the toxin.

Rabbit *B* was protected from toxin death by 1,500 units of antitoxin injected into the skin, indicating that this amount of antitoxin exceeded the localizing capacity of the skin, spilled over into the blood stream, and neutralized the toxin.

Rabbit *C* (control) was not protected from toxin death by 15 units of antitoxin injected into the skin, indicating that this relatively small amount of antitoxin was localized in the skin of the nonimmunized animal, and was thereby prevented from reaching the blood stream and neutralizing the toxin.

Rabbit *D* (control) was protected from toxin death by 20 units of antitoxin injected into the skin, indicating that this amount of antitoxin exceeded the localizing capacity of the normal skin, spilled over into the blood stream, and neutralized the toxin.

Here we have an illustration of the marked localizing capacity which the skin acquires as a result of a relatively limited degree of immunization. By arbitrarily considering the antitoxin units localized as retention, or localizing, units then:

1) The skin of a nonimmunized rabbit possesses a localizing capacity of 15–20 units.
2) The skin of a rabbit previously immunized by two injections of the antigen possesses a localizing capacity of 1,000 to 1,500 units.

TABLE 1

Localizing Capacity, Expressed in Units, of 13 Different Tissues in Immunized and Nonimmunized Animals*

Tissue	Immunized Animals	Nonimmunized Animals (Controls)
	Localizing Units	
Skin	1,000–1,500	15.0–20.0
Under skin	750–1,000	15.0–20.0
Peritoneal tissue	750–1,000	5.0– 7.5
Lung	400– 500	2.5– 5.0
Spleen	300– 400	2.5– 5.0
Uterine wall	200– 300	2.5– 5.0
Popliteal gland	200– 300	5.0–10.0
Testicle	200– 300	5.0–10.0
Liver	150– 200	2.5– 5.0
Articular space	150– 200	5.0–10.0
Skeletal muscle	75– 100	5.0–10.0
Brain	75– 100	5.0–10.0
Blood stream	50– 75	2.5– 5.0

* The rabbits were immunized by two injections of horse serum about one week apart.

Our experimental records show that if rabbits are immunized by 4 or 5 injections of horse serum instead of 2 injections, the skin of these animals will localize as many as 3,500 units of horse serum antitoxin, possessing what might be considered 3,500 localizing units.

The same experimental approach made possible the quantitative determination of the localizing capacities of 13 different tissues of nonimmunized and immunized animals (Table 1 and Charts 1 and 2). For reasons of simplicity, in order to reduce the localizing capacities of different tissues to single figures, the retention units for the tissues in the charts are based on the number of antitoxin units injected in each tissue that protected the animal against the lethal dose of toxin. The number of antitoxin units that did not protect the animals are the lower figures given in the table. It is evident from the table and charts that in nonimmunized animals the units of retention range from 5 to 20; in the immunized animals, the range is from 75 to 1,500.

The skin shows the greatest localizing capacity, probably because through evolutionary ages this tissue has been exposed to microorganisms. As a result, it has built up a marked capacity to localize them and thereby prevent them from entering the blood stream. This same localizing capacity is evidently manifested against protein antigen. The exposed mucous membranes apparently also show a marked localizing capacity. Studies of the localizing capacity of the conjunctiva indicate that this tissue shows a localizing capacity similar to that of the skin. The intraperitoneal tissues show an extensive localizing capacity, possibly because of the relatively large surface exposure to which the injected antigen is subjected.

It is of interest that of the internal organs studied, the lung shows the next marked localizing capacity, with 400–500 retention units, possibly because it is, in part at least, an exposed tissue. Skeletal muscle shows a relatively low retention capacity, with 75–100 retention units, and the same applies to brain tissue. The retention units are lowest, only 50–75, when the antigen is injected in the blood stream. This finding is also not without interest, since, when the antigen is injected in the blood stream of a nonimmunized animal, the retention units are only 2.5–5.

This tissue immunity experiment is presented to emphasize the importance of localization in immunity and to stimulate an interest in the study of individual tissues in immunity. The method employed is limited in its application. Yet it has brought to light, in addition to the data presented in

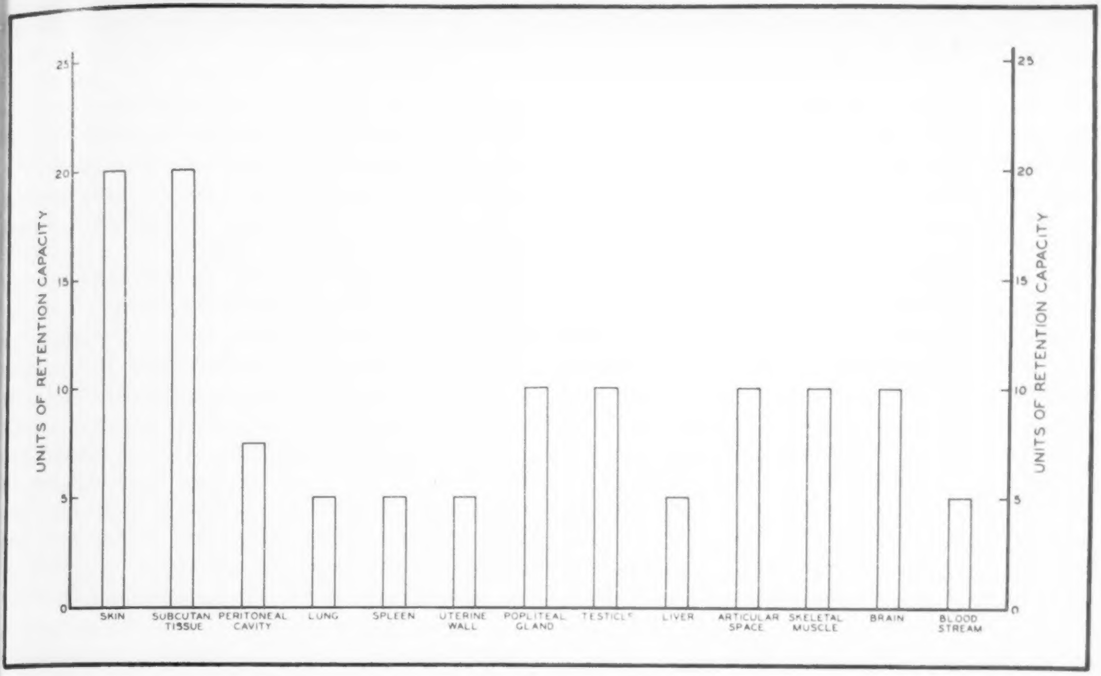


Chart 1. Retention, or localizing, capacity of different tissues of nonimmunized rabbits for protein antigen (showing a range of 5–20 units).

the table and charts, some experimental results of considerable interest. For example, the method has shown that the localizing phenomenon in the skin can be detected 48 hours after an immunizing injection. Since antibodies become detectable in about a week after an immunizing injection, it would

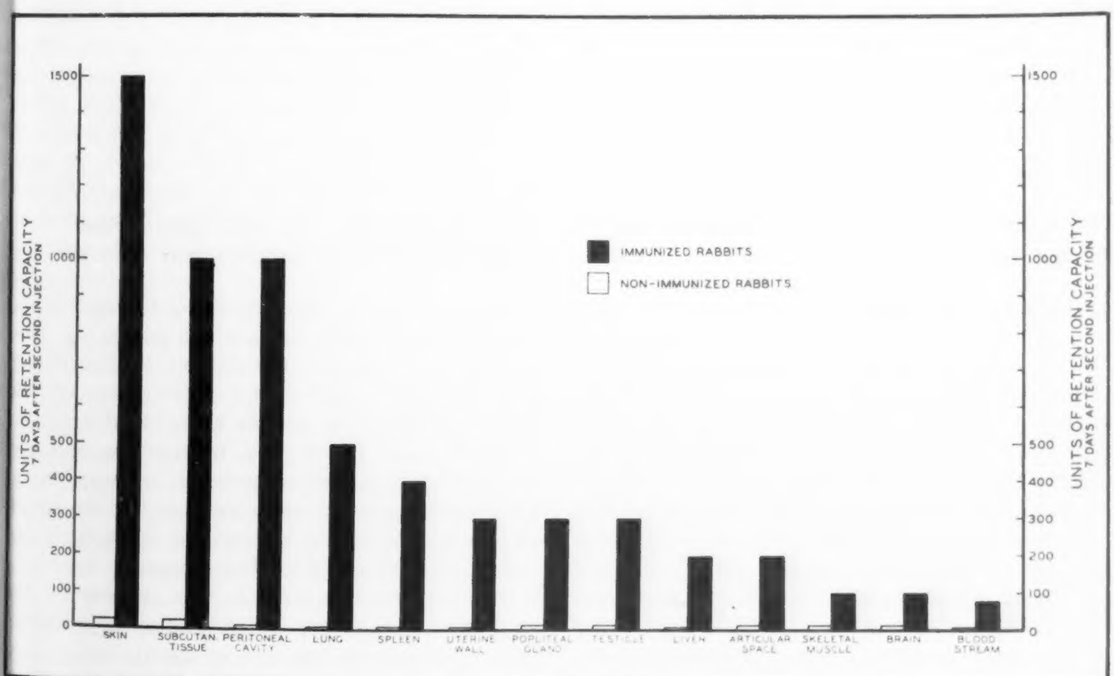


Chart 2. Retention, or localizing, capacity of different tissues of rabbits for specific antigen following the injection of two immunizing doses of protein antigen (showing a range of 75–1,500 units).

appear that the localizing phenomenon is perhaps one of the first manifestations in immunity.

The method has brought to light additional data with regard to the localizing phenomenon. For example, it showed that the tissues of young animals do not possess as marked localizing powers in immunity as the tissues of adult animals, which helps to explain the greater susceptibility of the young to infection. Also, when nonimmunized animals are injected with antibodies from immunized animals and are thereby passively immunized, the localizing capacities of their tissues do not show the same pattern as in actively immunized animals. The localizing capacities of the different tissues in the passively immunized animals are relatively similar in range. Thus, although the localizing capacities of the skin and skeletal muscle in these animals are increased over the capacities in nonimmunized animals, the localizing capacity of the skin is only about twice that of muscle, the same ratio as in nonimmunized animals. In actively immunized animals, however, as we have seen, the localizing capacity of the skin is more than ten times that of muscle.

This finding would suggest that the localizing capacity of the tissues in immunity is a function that manifests itself over and above circulating antibodies. What it is desired to emphasize, however, is that the study of the localizing capacities of different tissues in different animals under various conditions of immunization by the method herewith described opens up a highly promising field for the study of immunity. The method is an elaborate one: the data in the table and charts required some 200 rabbits. But if we are to fully understand immunologic reactions to microorganisms and other antigens, we must understand not only the reactions of the body as a whole but also of the individual tissues.

TURNING to the results in the tables and charts, it cannot be said that clinicians have not recognized differences in the immunologic response of different tissues. Suppose a person has a staphylococcus infection of the skin, commonly called a boil. Then suppose another person has an infection in muscle by staphylococci of the same virulence and the same number, an infection commonly referred to as an abscess. It is a matter of common knowledge to clinicians that the abscess is of far greater danger, for it might mean the establishment by the staphylococci of other foci of infection in different parts of the body. In the case of the boil, this possibility is altogether unlikely.

When we recall that the localizing capacity of skeletal muscle is only about one tenth that of the skin, it would be expected that the chances of escape by the staphylococci from the muscle infection would be far greater than the chances of escape from the skin. One must take into consideration, of course, that the skin infection is surrounded by a relatively thick fibrous wall, whereas the muscle infection does not have so pronounced a wall, which again emphasizes that different tissues play different roles in immunity.

After giving so much emphasis to the localizing capacities of the tissues in immunity, it might perhaps be well to consider briefly the tissues in the absence of immunity. According to the table and Chart 1, the localizing capacities of the tissues of the nonimmunized animal are apparently very slight, or perhaps negligible. This is evident not only from experimental findings in the laboratory but also from clinical findings in human beings. For instance, human beings lack immunity to the spirochete *pallidum,* the causative organism of syphilis. Hence, when the spirochetes gain entry into some area of the skin or mucous membrane, they soon reach the blood stream. A period of incubation then follows when there is no indication in the local area that a highly virulent microorganism has gained a foothold in the body.

The same holds true of tularemia. Human beings lack immunity to this disease. The result is that when the *tularense* microorganisms gain entry at some point in the skin, they are not localized at that point but soon reach the blood stream. What often happens is that a butcher will contract the infection while skinning an infected jack rabbit. He may have a scratch on his hand that facilitates his getting the infection. As the microorganisms gain entry through the scratch, there is at first no local nor systemic indication that highly virulent microorganisms have worked their way into the body.

Taking a third illustration, we might inject tubercle bacilli under the skin of a guinea pig. This animal lacks natural immunity to these bacilli, and when they are injected they rapidly escape from the area of injection and reach the blood stream.

Briefly, the tissues of a nonimmune host are unable to localize microorganisms or protein antigens and prevent them from entering the blood stream and becoming widespread throughout the body. This lack of the localizing capacity, however, is relative and not absolute. In relation to the marked localizing capacity of the tissues in immunity, the localizing capacity in the absence of immunity is apparently negligible. Of great interest is the fact that during the period of incubation in infection and immunity, virulent microorganisms

can circulate in the blood stream without apparent harm to the host.

When it comes to the question of how the tissues in immunity localize a specific antigen, perhaps a wise answer would be "I don't know." For we are entering here into the realm of theory, and sometimes good data are "spoiled" by wrong theories. I have been impressed at times by the view that a scientist should present data and omit explanatory theories; for the data will live—the theories might die the next day. At other times, I have been impressed by the view that a scientist must present theories explanatory of his data, else they are uncorrelated facts and, like the pieces of a jigsaw puzzle, meaningless—that a wrong theory is better than no theory. It might be best, therefore, to consider some theories.

Before presenting my own theory of the basis of localization, I wish to present the theories of two distinguished American pathologists. Opie, who has investigated the localization of injected protein in the skin of specifically immunized rabbits, came to the conclusion that the protein combines locally with circulating antibodies (Inflammation and Immunity, *J. of Immunol.*, 1929, **17**, 329). The end result of this combination is the formation of a precipitate in vivo, just as a precipitate is formed when antigen and antibody are brought together in vitro. Opie believes that the union of antigen and antibody in the area of injection is toxic to the tissue and the resulting injury calls forth a local inflammatory reaction.

This fact should be emphasized: namely, when localization of antigens or of microorganisms takes place in a tissue of an immunized animal, a local inflammatory reaction occurs. Menkin (*Dynamics of Inflammation*. New York: Macmillan, 1940), who has extensively studied many aspects of inflammation, believes that it is the local inflammatory reaction that is responsible for the localization of the antigen. It is known that inflammatory tissue possesses marked localizing capacity, not only for specific antigens but also for inert substances, such as dyes. Menkin's explanation overlaps to some extent that of Opie. The antigen is held in the area of injection by its union with circulating antibodies. The toxic effect of this union produces local tissue injury and, in turn, an inflammatory reaction. The inflammatory tissue then intensifies the localizing response.

Not being a pathologist, I should not presume to question the views of two distinguished workers in a tissue response associated with inflammation. To me, however, immunity is a physiologic function of all cells, and my experiments have been carried out only from this point of view. If, therefore, I must encroach upon the field of pathology by touching upon the inflammatory reaction to a specific antigen in immunity, my aim is to consider this reaction only in its broadest sense.

Actually, my experiments do not question the generally accepted view that inflammatory tissue possesses a marked capacity for localizing specific antigen and nonspecific substances. I differ from Opie and Menkin only in the question of the first and initial step in localization. My experiments indicate that the first step in localization is the result of some colloid-chemical union between the tissue cells and the antigens; that this union is similar to that which occurs between white corpuscles and bacteria in phagocytosis and between antigen and antibody preliminary to agglutination and precipitation in the test tube.

Briefly, I believe that as a result of immunization, all the body cells develop the property of identifying the specific antigen and of entering into union with it. Certain cells, such as reticulo-endothelial cells, possess this property to a greater extent than other cells. The cells of the exposed tissues of the body possess this property to a marked degree, but all body cells possess it to some degree.

Hence, when we inject microorganisms or other antigens into a tissue of an immunized animal and the injected material is localized or retained or fixed—to use different nomenclatures—in the area of injection, we see the end result of colloid-chemical union between the local tissue cells and the injected material. The same holds true of infection under natural conditions. If I, who possess natural immunity to streptococci, develop a streptococcic sore throat, it means that the surface cells of the throat, having come into contact with these microorganisms, have entered into chemical union with them and, as a result, localized them in such a way that they cannot enter the blood stream and produce a septicemia.

When tissue cells enter into chemical union with microorganisms or antigens and hold them localized, what effect might such union and localization have on the cells? The cells are bound to become injured, since their normal processes are interfered with. Just as the phagocyte, by holding bacteria in its body, pays with its life to protect the host, the tissue cells which hold microorganisms fixed to prevent them from entering the blood stream and becoming widespread evidently undergo injury and even death to protect the body as a whole.

The moment tissue injury occurs in a given area in the body, a chain of events takes place aimed at destroying the substance or substances respon-

sible for the injury and at healing of the injured tissue. Inflammation, which includes the dilation of the capillaries in the area, the accumulation of fluids and phagocytes, and the formation of a fibrous wall to circumscribe the area, is thus believed to be the sequel to the localization of microorganisms or antigen in a tissue of an immunized host.

Inflammatory reactions, occurring in connection with the localization of antigen, show different characteristics. For example, let us say we have before us a rabbit immunized by several injections of horse serum. The question is, What type of inflammatory reactions will the animal show to the injections of 0.01 cc, 0.1 cc, and 1 cc of horse serum? On injecting these amounts in different areas under the skin, it is found that 0.01 cc causes a small, slightly raised inflammatory area which reverts to normality in 48–72 hours; that 0.1 cc results in a larger inflammatory area which takes about a week to revert to normality; and that the 1-cc amount calls forth not only a relatively large inflammatory area, but the center of this area becomes black, indicating the cutting off of the blood supply, with resulting death of tissue. It takes some months before this area is fully healed.

The relationship between an inflammatory reaction, which pathologists have long emphasized to be a defensive response of the body to tissue injury and inflammation with tissue necrosis, interested me as a student of immunity. The death of the tissue of an immunized animal right in the center of the defensive inflammatory reaction seemed at first difficult to explain immunologically.

Let us then re-examine this experiment. The injection of 0.01 cc and 0.1 cc of horse serum under the skin of the immunized rabbit called forth local, raised inflammatory areas without any indication of tissue necrosis. Studies of the fate of the injected horse serum indicate that it is destroyed by the inflammatory reaction. When the inflammatory reaction subsides and the areas assume normality, the injected horse serum has disappeared. These results must mean that the animal was sufficiently immunized to call forth a defensive inflammatory reaction of such strength as to completely destroy these small amounts of horse serum and to heal the injured tissue.

Suppose the inflammatory reaction mustered by the immunized animal is insufficiently strong to destroy the injected horse serum? Undestroyed horse serum will then remain within the inflammatory area. The picture then changes from increased blood volume, associated with inflammation, to the cutting off of the blood supply, presumably by the clogging of the blood vessels with thrombi. Instead of a raised inflammatory area, a sunken-tissue necrotic area is noted.

This is what happened when, in our experiment, we injected an entire cubic centimeter of horse serum under the skin of the rabbit. The rabbit's immunity, strong enough to call forth inflammatory reactions to destroy 0.01 cc and 0.1 cc of injected horse serum, was not strong enough to destroy 1 cc, and the undestroyed horse serum within the inflammatory area changed the defensive inflammatory reaction to a local tissue necrotic reaction.

I have published three simple methods for changing a defensive inflammatory reaction to the injection of an antigen in an immune animal into a tissue necrotic reaction. We have just considered Method 1. Method 2 consists of calling forth a local inflammatory reaction in the skin of an immunized rabbit to the injection of a specific antigen, and then injecting a trace of the same antigen within the inflammatory area. The point where the specific antigen is injected will become purple and then black in a matter of minutes, indicating death of tissue. Method 3 consists of again injecting a relatively small amount of antigen in a tissue, such as the skin, of an immunized rabbit. An inflammatory reaction will appear at the site of the injection. A relatively large amount of the antigen is then injected into the blood stream. The attraction which the local inflammatory area exerts on the circulating antigen results in the accumulation of the antigen within the inflammatory area, and, when the amount accumulated becomes greater than the inflammatory reaction can destroy, tissue necrosis will set in within the inflammatory area.

These three methods are readily applicable to bacterial suspensions. For example, an injection of a small dose of vaccine of colon bacilli into the skin of a rabbit will call forth a raised inflammatory area which will subside to normal in a few days. A large dose of the same vaccine will result in an inflammatory reaction with central tissue necrosis. An inflammatory reaction called forth by a small dose of the vaccine will change to a tissue necrotic reaction by the injection of sufficient vaccine in the blood stream. Also, an inflammatory reaction resulting from a small dose of the vaccine can be changed to a tissue necrotic response at a given point if a small amount of the vaccine is injected directly into the inflammatory area.

This change from inflammation to tissue necrosis becomes understandable by introducing teleologic reasoning. In the struggle between a host and microorganisms, the host is on top, so to speak, as

long as his defensive mechanisms can localize and destroy the attackers. If the defensive mechanisms are incapable of destroying the microorganisms, the latter gain the upper hand in the local area, and, as a result, they destroy the tissues of the host. Since chemical antigens exhibit immunologic characteristics similar to those of living microorganisms, we see the same relationship between inflammation and tissue necrosis applied to protein antigen, such as horse serum. Briefly, an inflammatory reaction in immunity is a defensive reaction of a host against microorganisms (or antigens), and tissue necrosis within such an inflammatory reaction may be looked upon as a defensive reaction of the microorganisms (or antigens) against the host.

I HAVE tried to emphasize that when we lack immunity to given microorganisms or to protein antigens, our tissues lack the capacity to localize them to any degree in the area of contact. The result is that they gain entry into the blood stream in a matter of minutes. One of the first manifestations of immunity is the development of the capacity to localize the specific microorganisms or antigens in the area of contact, with the result that they do not gain access to the blood stream. The cutaneous tissue possesses a localizing capacity above that of other tissues, a situation which is understandable, since the greater the localizing capacity of the surface tissue, the greater the protection of the animal from blood-stream invasion.

Localization is a widespread response to microorganisms under natural conditions in infection. As long as the infected person can keep the microorganisms localized, provided they do not produce soluble toxin, he has the upper hand. When he loses this localizing capacity—and this happens when an infected person becomes overwhelmed by the microorganisms—death may ensue.

Localization, although protective to the body as a whole, is injurious to the local tissue, just as phagocytosis is protective to the body as a whole but injurious to the phagocytes. When a tissue in a given area localizes microorganisms, it holds onto them, presumably by colloidal-chemical union. Such union interferes with the physiologic function of the area and leads to injury of the local cells. The body then rushes aid to the injured cells by means of a local inflammatory reaction. The fluid and phagocytes that accumulate in the area are often sufficient to destroy the microorganisms or antigen; then healing of the local area takes place. If the inflammatory reaction is too weak to cope with the microorganisms or antigen, the situation is reversed in favor of the microorganisms or antigen, just as in warfare, if, in a given zone, the defensive efforts are incapable of destroying the entrenched enemy, the enemy causes the destruction instead.

With this approach to the phenomenon of localization and its aftermath, inflammation, let me describe an experiment on anaphylaxis in the guinea pig. A small amount of protein, such as horse serum, is injected in a guinea pig. The animal will show no apparent effect, since it possesses no natural immunity to the horse serum. Several weeks later a small amount of horse serum is injected directly into the blood stream. The animal almost immediately gives evidence of shock; it breathes with great difficulty and it dies from asphyxia within a few minutes.

How would you and I have interpreted this experiment? We would probably have assumed that the first injection of the horse serum in the guinea pig had immunized the animal. This immunity led, of course, to the development of the localization capacity for horse serum so as to prevent it from entering the blood stream. Hence, the injection of the horse serum directly into the blood stream antagonized this localizing mechanism and led to immunologic shock.

We would have concluded that the guinea pig's immunologic make-up is so delicately adjusted that it cannot be tampered with experimentally; that one cannot, by means of a syringe and needle, shoot antigen directly into the blood stream of that animal after building up immunologic mechanisms to prevent the antigen from reaching the blood stream. The animal's response is very much like that of a nation which, before the days of the airplane has built up an almost impregnable wall to prevent an enemy from gaining entry, and then suddenly discovers that the enemy has found its way into the very heart of the country. Shock and havoc would be bound to result.

Evidently, that country with the defensive wall which surrounds it was not in a state of the opposite of defense; namely, on the enemy side. It was in a defensive state, but its defense was not adequate to cope with an unexpected emergency situation. The same situation, it would seem, applied to the guinea pig. Its defensive mechanisms had been developed to cope with natural immunologic conditions, and it apparently lacked provisions to defend itself against this particular laboratory experiment. Immunized animals other than guinea pigs and human beings apparently have provisions to cope with injections given directly into the blood stream. This is very fortunate,

especially in the case of human beings, since, in diagnosis and therapy, methods of injection have become so widespread a practice.

If, then, there had been no concept of anaphylaxis, or "against defense," Arthus, very likely, would not have proposed a concept of local anaphylaxis to explain the local inflammatory reaction in the rabbit to repeated injections of horse serum. For the localization of the antigen in the area of injection and the subsequent inflammatory reactions are obviously defensive mechanisms.

Without traditional concepts of anaphylaxis and of local anaphylaxis, today's textbooks on immunology would probably present discussions of tissue immunity side by side with humoral and phagocytic immunity, emphasizing the defensive reactions of the tissues under natural conditions of infection and immunity in harmony with the reactions of defensive antibodies and phagocytes. Then there would be a presentation of the responses of a host to injections under various conditions, perhaps divided into two categories: the responses that parallel natural conditions of infection and immunity and the responses that do not parallel these conditions. It is in the latter section that the behavior of the guinea pig and of other animals—and indeed the behavior of man—to various forms of injection which result in reactions that occur only under conditions of injection would be considered. Then, finally, there would be a section on disturbances in the immunologic function, in which the allergies of man would be discussed.

With regard to allergy, in *Tissue Immunity* (Springfield, Ill.: Charles C Thomas, 1936, 681), I stated:

> Neither immunity nor allergy, in our opinion, can be fully understood without recognizing the tremendous burden of defense carried by the exposed tissues of the body. Through evolutionary ages these tissues have had the task of warding off microorganisms, and preventing their entrance into the blood stream and into the deeper tissues. Is it not significant then that these surface tissues should be largely involved in allergic disturbances of man? It is this great burden that these tissues carry, it seems to us, that is related to these disturbances. Like over-vigilant guards, the surface cells begin to treat harmless substances as though they were harmful microorganisms, and react with great intensity on slight provocation.

It would appear that the allergic person suffers from hyperactivity of the immunologic function. Hyperactivity of a physiologic function is one of the most common disturbances of man. Excessive activity of the thyroid gland (hyperthyroidism), excessive secretion of acid in the stomach (hyperacidity), and high blood pressure (hypertension) might be mentioned as examples. The allergic person is generally "hyperimmune" to a particular pollen or to some other substance, just as the immune person may possess immunity to one particular strain of microorganisms and not necessarily to others.

If I may look into the future, I would say that the immunologic manifestations of the individual tissues in relation to those of the body as a whole, and the factors that influence these manifestations, will be studied to an increasing extent. The relationship between the anatomic structure of individual tissues and their immunologic function has hardly been explored. Future students of immunity will wish to know more fully the anatomic bases for the immunologic differences of the various tissues. They will want to understand more fully the immunologic capabilities of the different tissues, as well as their immunologic disturbances, such as those manifested in the allergies. Infections and the allergies occur primarily in certain tissues; hence, the more that is known about the immunologic function of these tissues and the functions of antibodies and phagocytes, the better we shall be equipped to cope with these disturbances.

AN IDEAL PARTNERSHIP

L. R. CLEVELAND

Professor Cleveland, of the Biological Laboratories, Harvard University, received half the Annual Thousand Dollar AAAS Prize in 1924.

ANIMALS, like humans, form good, bad, and indifferent partnerships. Perhaps the best-known example where both partners derive more or less equal benefit from living and working together is that of termites, or white ants, and one-celled flagellate protozoa which live in their insect partner. This partnership is a very old one. It existed in certain wood-feeding roaches, or Blattidae, before termites were given off from this ancient group of insects, and it still exists in them today very much as it did 200–300 million years ago. It has changed little if at all since termites diverged from roaches prior to the Tertiary.

Some roaches, however, formed a partnership with bacteria, and this partnership, like that with protozoa, was probably passed on to termites in the course of evolution. In other words, the protozoan partners were not replaced by bacteria. So much more is known regarding the partnership of insects and protozoa that the partnership between bacteria and insects will not be considered here—although if better known, it might be very interesting. Since the partnership between protozoa and roaches is older and more nearly perfect from the standpoint of both partners, more attention will be given it.

Both roach and protozoa use the same food—wood. Almost any kind of wood, from hard and sound to soft and quite rotten, can be used; the main requirement is that it be fairly moist. The roach *Cryptocercus punctulatus* lives deep within the wood that it eats, in tunnels just wide enough for its body. All traffic is one-way. Movement is in, never out, until most of the log is consumed or until unfavorable conditions of one kind or another make it necessary to look for a new home. There is no housing shortage.

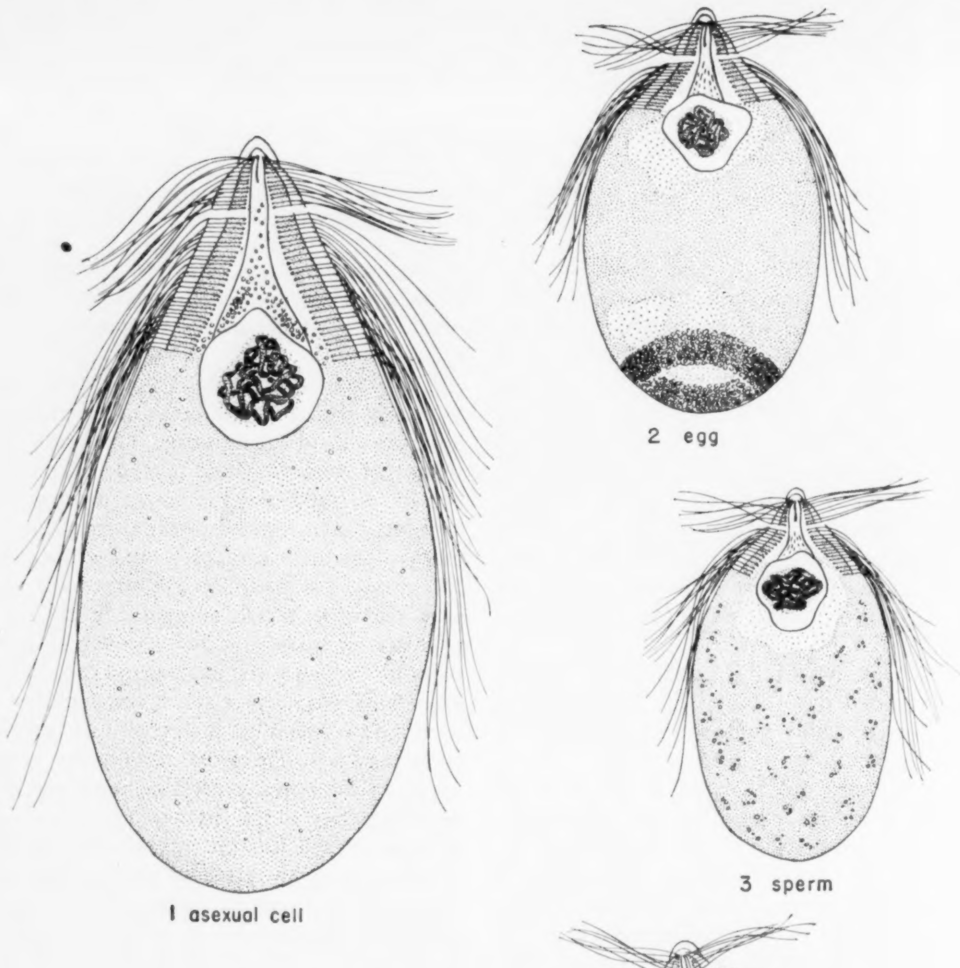
In such an environment, the roach, like the protozoa within it, lives in darkness. It comes out into the light only when searching for new and better living quarters; this, on the average, happens perhaps once every ten or fifteen years. Such secluded conditions assure both partners almost complete freedom from enemies. This, together with an abundant food supply and virtually no competition, would seem to offer important population control problems. Long ago, this was probably true, but, because of certain well-established adaptations made by both partners, the problem no longer exists.

The roach eats the wood and passes it on to the protozoa, which live only in the greatly enlarged hind portion of its intestine. The protozoa are so thick that room for more is made only when death occurs; medium-sized ones fit around big ones, and small ones around medium-sized ones, like so many watermelons, apples, and grapes in a bin. The roach has mouth parts which cut the wood into particles small enough for the one-celled protozoa to take into their bodies. If a few particles too large for the protozoa to take in should be swallowed, they never reach the protozoa because en route the roach has a screen in its crop for holding them back. There are three other valves elsewhere in the roach's intestine, each serving a different function in the partnership.

Even though this partnership has persisted for a very long time—possibly longer than any other—the two partners, in certain respects, still differ greatly physiologically. It is fortunate that this is so, for otherwise it would be impossible to experiment with them and learn what each does. If every change in environment that killed one killed the other, there would be no way to separate the partners and learn what each is capable of doing alone, and thus how each helps the other in the partnership.

Withholding not only of wood, but of all nutritious materials, affects the protozoa first and more adversely than the roach. This is especially true when asbestos moistened with water is fed the roach (great quantities are eaten, sometimes even when wood is also present). But this is not a satisfactory method of separating the partners; it will kill all individuals of some genera of the protozoa and most of those of other genera and still leave the roach alive. Yet it never kills all the protozoa, and, when the roach is returned to a diet of wood, the protozoan population begins to come back. Such an experiment lasts for many days, and, even though the roach does not die, it is injured considerably.

Since the roach can withstand a temperature 3°–4° higher than that required to kill all the


1 asexual cell
2 egg
3 sperm
5 fertilization 4

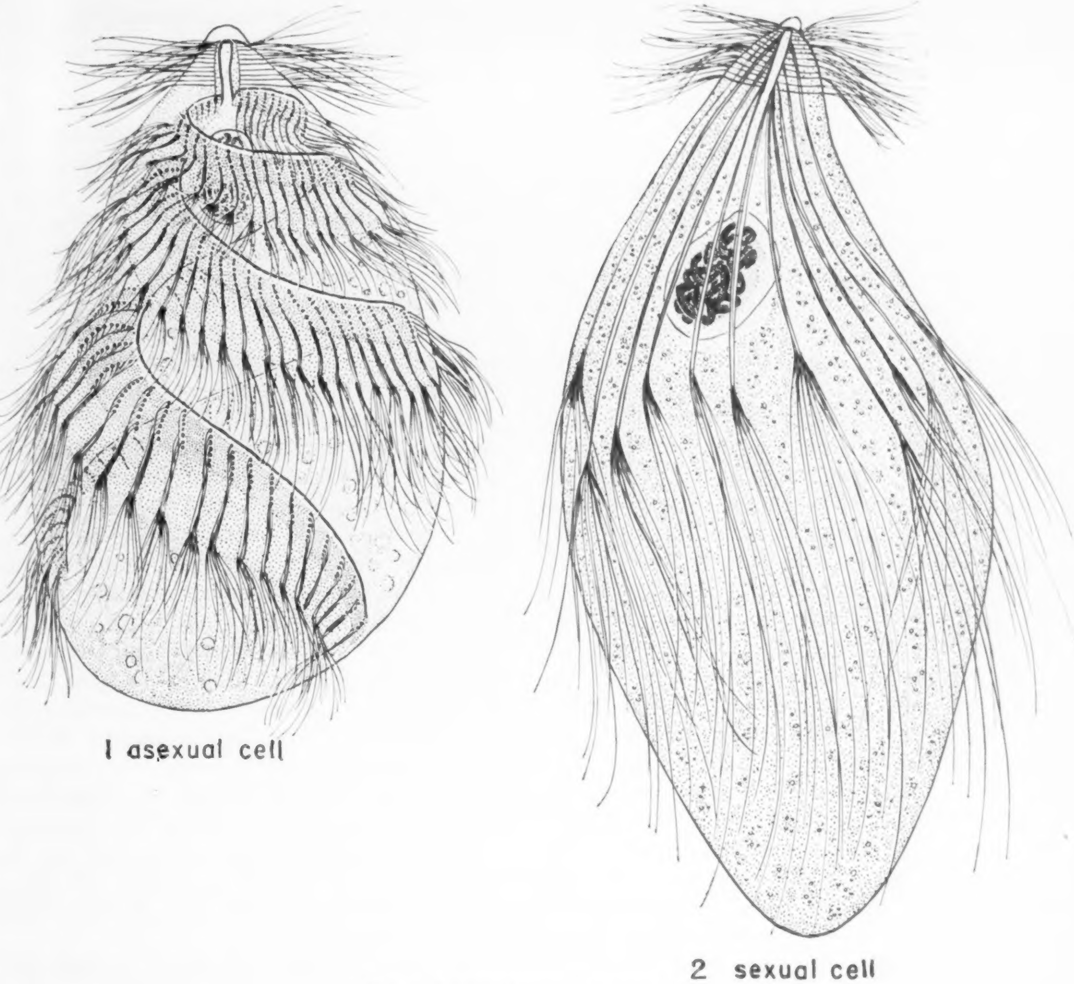


Fig. 2. *Leptospironympha wachula.*

protozoa within it, another type of experiment can be carried out in one hour or in twenty-four—depending on the temperature—and the roach is not injured by it.

An even more satisfactory method for killing the protozoa and leaving the roach uninjured is to use oxygen, which is very much more poisonous for the protozoa than for the roach. The time required to break the partnership depends on the concentration of oxygen used: two hours when four atmospheres are used; several days when one atmosphere or less is used. Temperature plays an important role in oxygen toxicity (a subject that cannot be considered here).

One can also obtain an egg case passed by a roach, keep it until the young hatch, and thus secure a population of roaches naturally free of protozoa. In termites the protozoa are killed every time their partner molts. By isolating termites and keeping them until they molt, one can obtain a population naturally free of protozoa.

Irrespective of the method used in breaking the partnership, the roach cannot live without its protozoa; the same is true of termites. And the protozoa cannot live in any other animal, nor free in nature—not in water, in soil, or in wood. Each partner is therefore absolutely dependent on the other for its existence. Many experiments have been carried out in attempts to learn the details of this dependency. Some facts have been established

Fig. 1. ***Trichonympha.*** 1, asexual haploid cell which under the influence of the molting hormone is converted in a single division into two unlike sexual cells (*1* and *2*); 4, sperm is entering egg through its ring of fertilization granules; 5, sperm has entered egg completely, is losing its organelles, and its cytoplasm has fused with that of the egg. Soon the nuclei will fuse and thus complete the process of fertilization.

quite clearly, but there are still many details regarding the precise nature of the partnership that are obscure.

Cellulase, the enzyme responsible for breaking down cellulose, the principal constituent of wood, is present only in the protozoa. When a roach or a termite loses its protozoa, either naturally or by experimental means, it loses its cellulase—no tissues of the body contain it. When the roach regains its protozoa, the cellulase reappears. One species of the protozoa has been grown in culture where it was able to produce cellulase indefinitely.

Both the roach and its protozoa have lived together for such a long time on a diet very low in protein that neither is able to survive if fed much of this substance. They are also unable to tolerate large amounts of carbohydrates other than cellulose. They are able, however, to live very well for many months on a diet of pure cellulose. This is probably due to the fact that the low rate of metabolism of the roach (seven or eight years to reach maturity) and the protozoa requires only a small amount of nitrogen, which the roach, like certain other insects, is able to conserve.

The roach can pass the protozoa on to its young only at the time of molting; hence, molting and hatching of the young must occur at the same time. Otherwise the young will die—and a fair number do—because the partnership is not established.

At each end of that portion of the roach's intestine known as the hind-gut, there is a valve, or screen, which will let the protozoa enter but will not permit their exit in either direction except during molting, when these structures are discarded and replaced by new ones. Thus, during the fall, winter, and spring months, when no young are produced, no protozoa are allowed to pass out with fecal material. From the standpoint of the protozoa, this is highly desirable, because if they passed out at a time when no young were available to take them on as partners it would mean certain death. From the standpoint of the roach, it is also desirable, because no useful purpose would be served if it released its partners when it had no young to take them on. This system of valves, then, works to the mutual advantage of both partners.

A third valve, which is situated in the middle of the hind-gut, and which has a powerful muscular pump to operate it, serves to force the substances elaborated by the protozoa from the wood into the mid-gut, where they come in contact with epithelial cells that absorb them. These substances cannot be absorbed in the hind-gut because it is lined with chitin. In forcing the substances from the hind-gut to the mid-gut the pump does not carry the protozoa along, nor does it carry wood particles; a valve between the two regions of the intestine permits only fluid to pass from one to the other anteriorly.

At the time of molting, and at no other time, some of the protozoa, particularly those in the lower portion of the hind-gut, become more resistant to external conditions and are thus able to survive outside the body long enough to ensure their transmission to the young roaches shortly after they hatch.

In termites the partnership operates differently. In the first place, the protozoa can leave the hind-gut of the termite any time. Ordinarily, they are not passed out with the fecal pellets, but are in the liquid material that sometimes leaves the intestine shortly after a pellet is passed. Instead of resistant forms of the protozoa being produced at molting, the protozoa die and the partnership has to be re-established following each molt. In termites, then, there is no need to correlate hatching of young and molting. This is an advantage in that the young may be hatched over a longer period, but it is a disadvantage to have to pass on the protozoa at hatching and at every molt thereafter. Also, more casualties occur than in roaches. However, the handicap of having to re-establish the partnership so many times in the life of a single termite is more than made up for by the fact that termites —with one exception—do not produce egg cases. Instead, they produce huge queens, virtual egg-laying machines, capable of laying countless thousands of eggs.

Perhaps the most interesting feature of the partnership is the reaction of both partners to the same hormone. In the roach it produces molting and thus allows the insect to increase in size; in the protozoa, it produces several types of sexual behavior, some of which, incidentally, throw considerable light on the origin and evolution of sexual processes in general. Some genera of the protozoa have benefited from this phase of the partnership, since the hormone causes them to produce gametes and thus enables them to utilize whatever evolutionary advantages biparental inheritance offers. The advantage must be considerable since this roach has seven families, twelve genera, and twenty-seven species of protozoa in it.

This is the first time the secretions of the cells of one animal have been shown to possess the ability to convert the asexual cells of another animal into sexual cells and thereby exert a direct influence on the course of its evolution and development. Other examples similar to this one will probably be uncovered by further investigations.

The life cycles and evolution of many parasitic protozoa have probably been influenced to a considerable extent by their hosts.

The illustrations show the great nuclear and cytoplasmic changes that occur in a cell when it is under the influence of the molting hormone. In the genus *Trichonympha,* for example, an asexual cell, by a single division of its nucleus and cytoplasm, produces two gametes, a sperm and an egg. The sperm enters the egg, and its entire contents, cytoplasmic and nuclear, fuse with those of the egg to form a new individual (Fig. 1, 1–5). Each new individual undergoes two meiotic divisions within twenty-four to forty-eight hours after its formation. This serves to return the chromosomes to the normal haploid number and to replace quickly those protozoa given to the newly hatched roaches.

In the genus *Leptospironympha* even greater changes occur in the transition from asexual to sexual cells. The large spiral flagellar bands of the asexual cell are entirely absent in the gametes (Fig. 2).

These sexual forms of protozoa are never produced except when the roach molts. Heating its head inhibits the production of the hormone which makes it molt. If kept at a constant temperature of 18° C, a roach never molts. A brief period of chilling—an artificial winter—is necessary.

It is thus possible by one experimental procedure to inhibit indefinitely molting and the production of the sexual forms of the protozoa, and by another to produce molting and the production of sexual forms of the protozoa more frequently than occurs naturally.

COWBIRD

Small parasite,
why do you leave your eggs
in other nests?
Can't you nurse your own,
or must you tend the cattle in the fields,
leaving your offspring
to a phoebe's ire,
or to a vireo's generous tolerance?
Must you think only of your
own bright needs?
You feed on grasshoppers,
or on the seeds
of grain or foxtail grass,
while in some alien nest
a small brown head
wakes to a foster breast!

MAE WINKLER GOODMAN

NERVES IN VIVO

CARL CASKEY SPEIDEL

The Annual Thousand Dollar AAAS Prize was awarded to Dr. Speidel, professor of anatomy, University of Virginia, in 1931.

NINETEEN years ago I published a paper on the behavior of regenerating tissues in frog tadpoles that were treated with thyroid gland extract. During that investigation certain puzzling pigment tips were noted. In attempting to watch the origin and fate of these tips in living animals under the microscope, my attention was soon diverted to nerves in the vicinity. Since that time a study of tadpole nerve fibers in vivo has been my major research interest.*

Early investigations revealed the details of nerve growth and regeneration. Direct microscopic observations showed how the pioneer nerve sprouts grew out, how the satellite sheath cells migrated outward along the nerve sprouts and multiplied, and how the nerve sprouts and sheath cells then cooperated to form an insulating sheath of a fatty substance called myelin. In all this work it was found possible to keep track of individual nerve fibers and sheath cells. (The technique was described briefly in an article for this journal: **37**, 47–49, 1933.)

Further investigations continued along several lines: an experimental study was made of the reactions of nerve fibers to various degrees of irritation and injury, together with the steps of recovery or of degeneration. Among the injurious treatments or agents used were electricity, X-rays, heat and cold, salt solutions, strong anesthetics, endocrine gland extracts, alcohol, metrazol, mustard compounds, acids, alkalies, sulfa drugs, and the infliction of wounds by cutting or bruising.

A separate paper was devoted to the effects of alcohol on nerve fibers, and another one to the effects of metrazol. Particular attention was also paid to the behavior of nerve endings. The modern use of metrazol, insulin, and electric shock in the treatment of some human mental disorders made these studies of tadpole nerves of timely interest. Direct evidence was secured that such treatments could cause alterations in the distribution of nerve endings in tadpoles. This suggested a structural basis for the change in the mental condition of human patients similarly treated. (The principal adjustments of nerve endings in general were summarized in a paper presented before The Harvey Society in 1941.)

Nerve fibers are intimately related to other tissues, and it seemed advisable to study these also. A great deal of time was devoted to striated muscle. The same technique of observation was used to watch the growth and regeneration of individual muscle fibers, the minute changes in cross striae during contraction and clotting, and the steps of recovery or degeneration following various grades of injury after different kinds of treatment.

Other kinds of cells were studied also, though less intensively. These included the cells of the blood vessels and blood, lymph vessels, connective tissue, epithelium, notochord, and pigment.

Early attempts were made to record the growth of nerve fibers by fast-motion or time-lapse movies. Although difficulties in technique were encountered, some very satisfactory cine-photomicrographs were obtained. These were first exhibited in 1932, one of the first exhibitions being given at the meeting of the AAAS in December of that year at Atlantic City. Since that time I have tried steadily to build up a motion-picture film library of various kinds of cellular activities. Pictures of this type are superb for demonstration purposes. (The method of making fast-motion microscopic movies of tadpole tissues was described in an article appearing earlier this year in the *American Scientist*.)

NERVES AND SENSE ORGANS

For the past few years my attention has been devoted chiefly to the relation between nerves and special sense organs. A study of these has shown clearly how one structure may affect another in some subtle manner, and how regressive changes will ensue if the normal relationship is eliminated.

The frog tadpole's tail contains a series of special sense organs called lateral-line organs (Fig. 1). They are located in two lines, one along the middle of the tail, the other in the dorsal fin. They look much like the taste buds of the human tongue (Fig. 2). They are sensitive to chemical variations in

* Aided by grants from the National Research Council (Committee on Grants-in-Aid); American Medical Association (Committee on Scientific Research); American Association for the Advancement of Science; Virginia Academy of Science; and the Penrose Fund, American Philosophical Society.

the water in which the tadpole lives and also to vibrations and to currents. They are innervated by branches of the vagus nerve, a cranial nerve. In both frog and man this vagus nerve also serves the heart, lungs, stomach, and other important structures.

Vagus nerve growth and innervation of organs following tail-tip removal. If the tip of the tail is cut off, regeneration soon takes place and a new small tip develops rapidly. Into this grow fibers from the cut vagus nerve stumps and cells from the organs nearest the wound. A few days is sufficient for the lateral-line branch of the vagus nerve to give rise to very active sprouts (Fig. 3). The growth of this nerve is quite like that of the spinal nerves in the vicinity. Motion pictures beautifully portray its progress.

Ordinarily, the regeneration of the cut lateral-line nerve proceeds concomitantly with regeneration of organs, the latter arising from a placode,

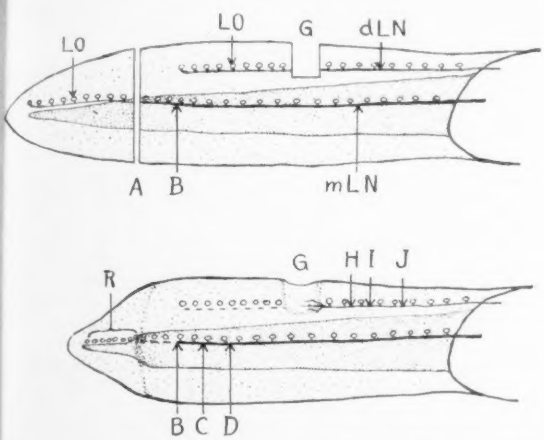


Fig. 1. Lateral view of frog tadpole's tail showing nerves and sense organs of lateral-line system and sites for operations.

Upper sketch shows position of the two lines (*small circles*) of lateral-line organs (*LO*); and their accompanying nerves, the main lateral-line nerve (*mLN*); and the dorsal lateral-line nerve (*dLN*). (Similar nerves and organs on left side not shown.) The tip of the tail may be cut off, as at *A*, and at the same time the main lateral-line nerve may be transected at *B*, thus denervating organs to the left. In like manner, a gap (*G*) cut in the dorsal fin causes denervation of organs to the left in this region.

Lower sketch shows condition of tail about 10 days later. The tip has regenerated somewhat, and a new line of organs (*R*) has grown into it. Beyond *B* the nerve has degenerated (*broken line*); *C* and *D* represent points at which additional successive nerve transections are made to keep terminal organs denervated. In the dorsal fin the gap has filled with regenerating tissue into which nerve fibers are growing from the right. Degenerating portion of severed nerve (*broken line, left*) and (*circles*) the accompanying denervated organs. To ensure continued denervation of these organs, additional transections of nerve may be made successively at *H*, *I*, and *J*.

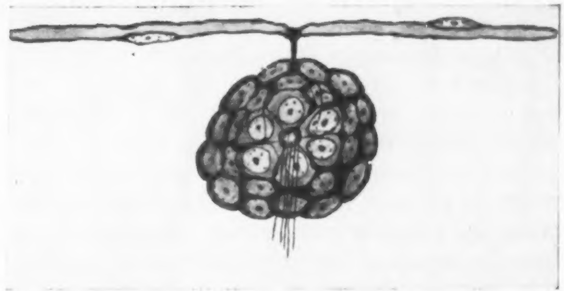


Fig. 2. A lateral-line sense organ and its nerve supply viewed microscopically from lateral surface of tail. In this organ 6 sensory cells are visible near center, grouped about a shallow pit. Five of the sensory cells are provided with long sensory hairs, which jut out through a central pore into the water. Except at the pore where the sensory cells are located, the whole organ is covered with a thin flat layer of skin cells (not shown). From the accompanying nerve fiber, between the 2 myelin segments illustrated, comes a side branch which enters the organ and divides into sub-branches, each of which terminates in a delicate end bulb. (This sketch, semischematic in nature, combines structures which under the microscope are actually at various levels of focus.)

or cord of cells, that grows from the last organ into the newly developing tail tip. Normally, the nerve sends side branches into most of the new organs shortly after they become differentiated from the placode. Careful observation in a few favorable cases reveals some of the details (Fig. 4). Delicate naked nerve endings grow into the organs and become intimately related to the special sense cells. They function to receive sensory impulses from the organs, and these impulses are then conducted to the brain by way of the lateral-line and vagus nerves.

For the past 70 years investigators have been trying to determine the extent to which sense organs depend upon their specific nerve supply for their structural integrity. Two sharply contrasting views are held, derived from research work on both lateral-line organs and taste organs. One view emphasizes the paramount importance of the nerves for the origin, growth, regeneration, and maintenance of the organs. The other view stresses the independence of the organs and holds that the specific nerves are unnecessary for the origin of new organs, or for their growth, regeneration, and maintenance. My recent experiments on tadpoles, however, indicate clearly that neither of these views is wholly correct.

Regeneration of denervated organs. A very effective method of determining whether lateral-line sense organs can arise without lateral-line nerve influence was used. This involved the clipping off of the tail tip, combined with several successive transections in an anterior direction of

the lateral-line nerve (cf. Fig. 1). In this way, while the organs nearest the line of tail amputation were kept denervated, they were at the same time stimulated to strong regenerative activity. The first of the nerve transections could be made before, after, or at the same time as the tail-tip amputation. There was no difference in the end result. In all cases, the regeneration of new organs proceeded normally from placode cells which arose from the organs at the wound edge (Fig. 5). Mature organs with sensory hairs and other characteristic specialized features were formed. Such organs without nerve supply could persist for at least several weeks, until the time for metamorphosis with accompanying reduction of the tail brought the observations to an end. This type of experiment showed clearly that the specific nerve supply was not necessary for the regeneration and early growth of the lateral-line organs.

Maintenance of denervated organs. Experiments were next undertaken to find out whether nerveless organs would preserve their structural organization indefinitely, or whether regressive changes would ultimately ensue. Green frog tadpoles were used because these remained in the tadpole stage for one or two years. Individual organs in the dorsal fin region (cf. Fig. 1) were watched for periods of 3–21 months.

Histories of hundreds of organs in many different tadpoles showed that organs rendered nerve-

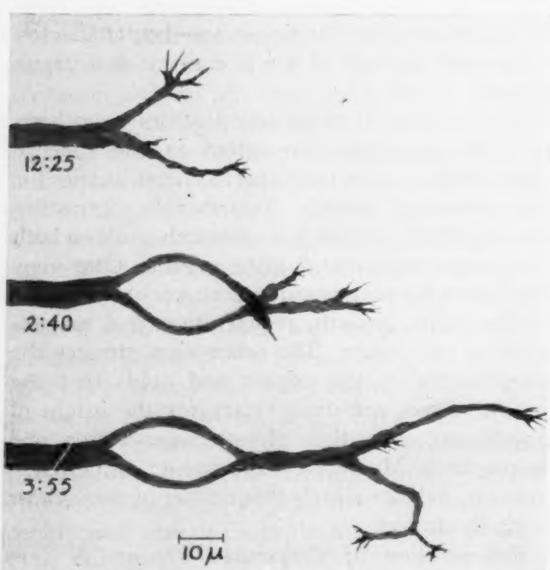


Fig. 3. Regenerating fibers of the lateral-line nerve, 3 days after removal of the tip of the tail. The sketches, made from motion-picture films, show 3 positions of the growing nerve sprouts over a 3½-hour period: at 12:25 P.M., 2:40 P.M., and 3:55 P.M., respectively.

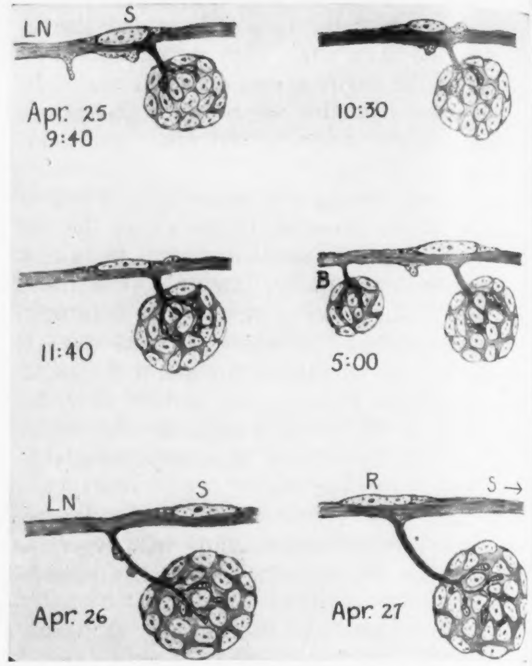


Fig. 4. Lateral-line organ innervation in a rapidly regenerating zone (from Speidel, C. C., *J. Comp. Neur.*, 87, 1947). Following tail-tip removal on Apr. 19, regeneration of organs and nerves took place as a new tip grew out. On Apr. 25, at 9:40 A.M., a young organ was visible, supplied by a short branch from the accompanying lateral-line nerve (*LN*). A sheath cell (*S*) was nearby. Growth changes in the nerve endings within the organ were noticeable during the next 7 hours, as shown at 10:30 A.M., 11:40 A.M., and 5:00 P.M. Between 11:40 A.M. and 5:00 P.M. there also arose a new branch (*B*) which supplied a new small organ just becoming differentiated from regenerating placode cells in this zone. On Apr. 26 and 27 further growth of the nerve endings was discernible. At this time the organ was supplied with 4 sensory hairs. (These were located at a more superficial level of focus and are not included here.) Sheath cell *S* moved to the right, out of the field; sheath cell *R* moved into the field from the left. (A motion-picture record of this case was obtained.)

less for periods up to about three months did not necessarily degenerate. For longer periods, however, regressive changes became increasingly noticeable. These included atrophy (reduction in size), dedifferentiation (loss of some features of specialization), and degeneration (death of the component cells).

The case illustrated here (Fig. 6) strikingly demonstrates the long-range trophic effect of the specific nerve supply on the sense organs. In this tadpole, after an operation on the dorsal fin which severed both dorsal lateral-line nerves, regenerating fibers from both right and left proximal stumps entered the distal stump of the left side but not of the right side. A comparison was possible, therefore, between the reinnervated organs on the left

and the nerveless organs on the right. Significant differences were not apparent for the first 3 months, but they became increasingly conspicuous during later months.

The two stages illustrated, 1 month and 10 months, respectively, after the operation, show that whereas there was a normal growth increase on the reinnervated left side from a total of 28 to 50 organs, there was a marked decrease on the denervated right side from 23 to 8 organs. This number on the right side decreased to 7 organs before the observations were terminated nearly a year after the operation; at the same time the number on the left increased to 66. Outside the zone of operation on both sides proximal to the cut, where the organs were normally innervated, their number also increased.

Most of the surviving nerveless organs on the right side exhibited marked atrophy and varying degrees of dedifferentiation. The organs and their component cells were smaller. The sensory hairs were for the most part shorter and fewer in number, though there was a good deal of variation.

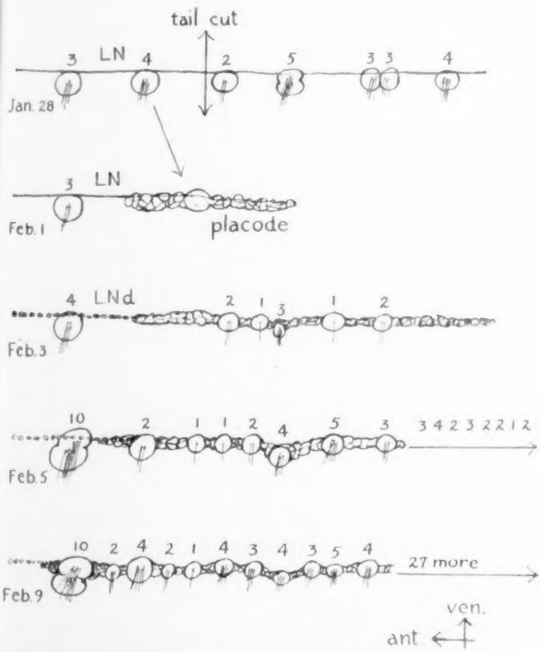


Fig. 5. Regeneration of denervated lateral-line organs (from Speidel, *J. Comp. Neur.*, **87**, 1947). The tail tip was cut off on Jan. 28 at the site indicated (*double arrow*). Three days later organ nearest wound had become transformed into a placode, or cord of cells. This was denervated and kept denervated by nerve transections farther anteriorly on Feb. 1, 3, 6, and 9. By Feb. 3, five new organs were visible. (The numerals designate the number of sensory hairs in each organ.) By Feb. 5 number of new organs had increased to 15; by Feb. 9, to 37. The average number of sensory hairs per organ also increased.

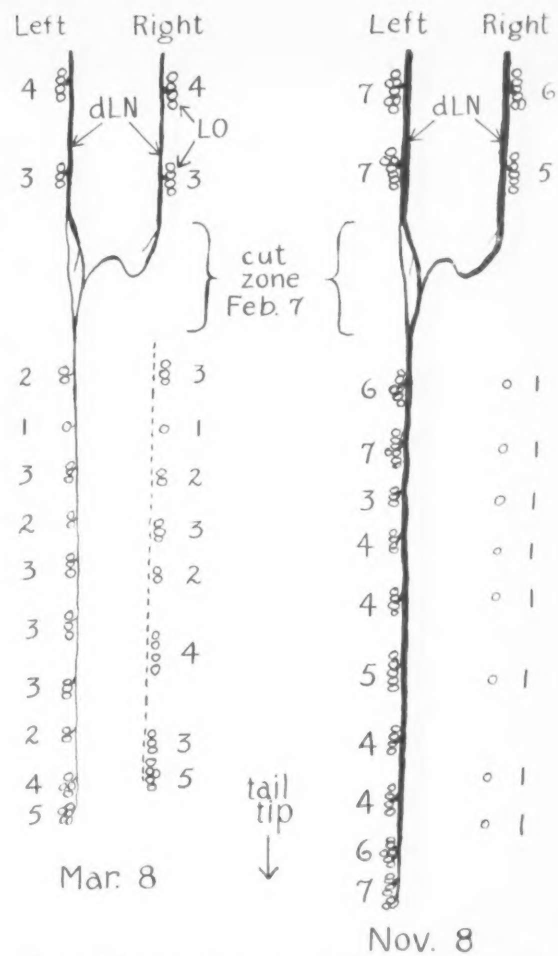


Fig. 6. Diagram of the dorsal lateral-line nerves and organs of both right and left sides showing the long-range changes in organ numbers in denervated and reinnervated zones (from Speidel, *Am. J. Anat.*, **82**, 1948). The nerves (*dLN*) and organs (*LO*) are represented as if seen from the dorsal aspect. A gap was cut in the dorsal fin on Feb. 7 at site indicated (*brace*). Cf. Fig. 1, gap *A*. By Mar. 8 regenerating fibers from both proximal stumps had entered the distal stump of left side only. On this side distal to the wound there were 28 organs in 10 groups; on the right side there were 23 organs in 8 groups. By Nov. 8, however, there were 50 organs on the reinnervated left side, but only 8 single organs on the nerveless right side. Proximal to the cut zone the number of organs increased on both sides.

Thirteen other case histories were obtained in which regenerating nerve fibers reinnervated the distal stump and organs of one side only. The results were like those of the case just described.

Two interesting case histories of partial reinnervation were obtained from tadpoles which were kept under observation for record times of 21 and 19 months, respectively, after operation. In one of these (Fig. 7) the regenerating nerve fibers on the right side crossed the wound and reinnervated the

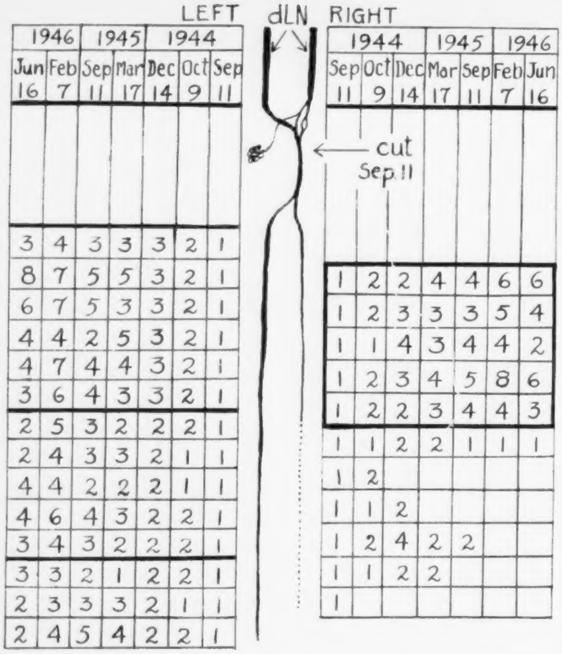


Fig. 7. History of lateral-line organs following partial reinnervation of the distal nerve stump on one side and complete reinnervation on the other (from Speidel, *Am. J. Anat.*, **82**, 1948). Both right and left dorsal lateral-line nerves (*dLN*) were severed by a gap cut in the dorsal fin on Sept. 11, 1944 (*arrow*). Second and third cuts were made nearer the root of the tail on Sept. 14 and 18 (cf. Fig. 1, gap *G* and cuts *H* and *I*). Regenerating fibers from each proximal stump grew into scar zone and formed a common trunk, which divided and gave branches to each distal stump. On left side, entire nerve stump and its associated organs became reinnervated. On right side, however, the innervating fibers, 2 in number, grew only halfway through the distal stump and reinnervated organs in this zone only. The terminal half of right distal stump and the organs of this zone remained completely devoid of nerve fibers (*dotted line*). On Sept. 11, 1944, there were 11 single organs on right side and 14 on left. (The numeral in each block indicates number of organs in a group.) The changes in organ number are shown for Oct. 9 and Dec. 14, 1944; for Mar. 17 and Sept. 11, 1945; and for Feb. 7 and June 16, 1946.

proximal 6 groups of organs only, leaving the distal 6 groups on the same side quite nerveless. On the left side all 14 groups of organs were reinnervated. During the following months the organs supplied with nerves steadily multiplied. The greatest number was reached about seventeen months after the operation. This increase was from an original number of 14 on September 11, 1944, to 68 on February 7, 1946. (After February 1946 the number of organs decreased somewhat as the time approached for metamorphosis with accompanying tail resorption.) In sharp contrast, during the same period the nerveless organs on the right side decreased from 6 to 1.

In the other case history of partial reinnervation, which is not illustrated here, a single regenerating nerve fiber grew across the wound zone and reinnervated the proximal 7 groups of organs on one side, but not the distal 9 groups on the same side. On the other side, there was no reinnervation of any of the 14 groups of organs across the wound. Nineteen months after the operation only 1 organ of the entire 23 nerveless groups survived, as contrasted with 15 organs that composed the 7 groups innervated by the single nerve fiber.

These two case histories, as well as that given in Figure 6, bring out also that there is a significant correlation between the number of organs in a group and the number of innervating nerve fibers. Thus, a single nerve fiber is capable of supporting groups of organs which may attain a maximum average of 2.4 organs per group. Two nerve fibers are capable of supporting groups of organs which may attain a maximum average of 5 organs per group. Three or more nerve fibers may support a maximum average of more than 6 organs per group.

The foregoing examples show beyond doubt that the lateral-line nerve fibers exert a strong long-range trophic influence over the sense organs that they supply. Permanently denervated organs deprived of this influence cease growing after a time, and then undergo regressive changes leading to ultimate degeneration.

No evidence has been seen to indicate that nerves may induce sense organs to form from indifferent epithelium, a view which is held by many investigators. On the contrary, in these experiments organs arise only from pre-existing organs, or from the partially dedifferentiated remnants of organs.

Fate of organless nerves. Regenerating lateral-line nerve fibers in wound zones occasionally go astray and fail to reach any lateral-line organ. These may be termed "organless nerve fibers." Such fibers, during the first month or two, may become ensheathed with myelin and appear to be mature fibers; nevertheless, they suffer regressive changes later. These changes usually become conspicuous 3–6 months after the operation. The myelin sheath becomes less massive, and the enclosed axis cylinder becomes smaller. Then the myelin is lost entirely, and ultimately the nerve substance itself degenerates. Thus, the preservation of the normal structure of lateral-line nerve fibers appears to be significantly correlated with sense-organ innervation. In other words, sense organs exert an important trophic influence on their nerves.

SOME ACCOMPLISHMENTS AND LIMITATIONS OF REACTION RATE THEORY*

HENRY EYRING

Professor Eyring, of the Graduate School, University of Utah, received the Annual Thousand Dollar AAAS Prize in 1932.

THERE are two extreme types of reaction rates. One kind is typified by ordinary radioactive decay where the rate is unaffected by ordinary changes in temperature and pressure. The second is ordinary chemical reactions dependent on both temperature and pressure. For both types of reaction, the rate can be represented as

$$rate = \Sigma_i n_i \nu_i \gamma_i \qquad (1),$$

where n_i is the number of reacting systems in the ith state, ν_i is the number of times per second that the system in the ith state vibrates in the direction normal to the reaction barrier, and γ_i is the chance on each assault that the system successfully traverses the barrier.

If the rate is not affected by temperature or pressure, the values of n_i are in general known from the chemical analysis of the material. For equilibrium systems sensitive to temperature and pressure, the appropriate statistical mechanical calculation is required. This calculation, as well as the calculation of ν_i and γ_i, depends on adequate knowledge of the potential energy surface, especially in the neighborhood of the minimum and the barrier saddle point. In principle, it is possible to calculate all such surfaces for molecular complexes. For nuclei, potential surfaces are only known in the region where Coulomb's law holds. For distances closer than about 3×10^{-12} Angstroms (depending somewhat on the atom), the unknown nuclear forces determine the shape of the surface. However, Condon and Gurney and Gamow showed how barrier leakage could explain the radioactive decay of the elements and the capture of a swiftly moving particle colliding with the nucleus.

The fraction of molecules decaying per second radioactively was found to be approximately

$$k = \frac{1}{a}(2\mu E)^{\frac{1}{2}} e^{-\frac{4\Pi}{h}\int (2\mu(V-E))^{\frac{1}{2}} dx} \qquad (2).$$

Here a is the width of the interior of the atom. E is the energy of the escaping particle, μ is the corresponding reduced mass, and V is the potential energy of the barrier. The integral is to be taken from the point of entering the barrier to where it emerges. This treatment is satisfactory in so far as it can be tested without a better knowledge of the nature of the nuclear forces.

Before the above treatment was applied to the nucleus, an analogous treatment of molecular isomerization was made by Hund. Actually, the integral for barrier penetration in equation (2) is roughly the same magnitude for molecules as for the nucleus. This is because, although the barriers are about a million times as high for the nucleus, they are also usually over a thousand times thinner. The result is that chemical reactants almost invariably find it easier at ordinary temperatures to surmount the barrier rather than to tunnel through it. The most notable exception is the case where the three hydrogen atoms in ammonia first form a pyramid with the hydrogen base on one side of the nitrogen apex and then on the other. This turning wrong side out happens about 10^{10} times per second and is a true tunneling very much like radioactive decay. Examples of this kind are extremely rare, however. Most reactions involve the surmounting of the barrier. In 1928 London showed how the quantum mechanics could be used to construct potential barriers and pointed out that, in general, the surmounting of the barrier usually proceeded without electronic reorganizations of the kind occurring in electronic transitions.

Polanyi and Eyring devised means of constructing potential surfaces from London's approximate formula. These were sufficiently accurate to show that tunneling is in general negligible in chemistry and to form the basis for a quantitative statistical theory of reaction rates. In principle, we can presumably calculate the rate of any chemical change. In practice, it has only been possible so far to show from first principles that, for the simplest of reactions, $H + H_{2\,para} = H_{2\,ortho} + H$, the activation energy is less than 19 K calories, whereas experimentally it is found to be about 7 K calories. Nevertheless, we now have a detailed theory of reaction rates. Our only really serious limitation in predicting reaction rates a priori is the lack of workable methods for calculating barrier heights. This is a difficult problem, but it is of sufficient importance that it will surely be solved eventually.

* Owing to lack of space, references have been omitted.

The methods of perturbation theory lead to the London-type formula, which gives the energy for a system of atoms in terms of the binding energy between pairs. The binding energy between atom pairs is considered to be of two types: a classical "coulombic" binding, which is about 14 percent of the total and is simply additive; and the "exchange," or valence, binding which constitutes the remainder.

The well-known approximate London formula for the energy E for four monovalent atoms is

$$E = Q - \{\tfrac{1}{2}(a-\beta)^2(a-\gamma)^2(\beta-\gamma)^2\}^{\frac{1}{2}} \qquad (3).$$

Here the total coulombic binding is included in Q, a may be taken as the sum of the exchange part of the two initial bonds before reaction, and β is the sum of the two bonds formed after the reaction; γ is the sum of the two bonds which are present only in the activated state along with a and β.

The radical in (3) may be shown to be equal to the length of a vector found by adding β onto the end of a at an angle of 60° and γ onto the end of β, making the 60° angle which will make the vector sum smallest. A chemical reaction starts with a large and with β and γ zero and ends with β large and a and γ zero. The activated state will thus come approximately when $a = \beta$ and the total binding will be less than a or β because of γ. For three monovalent atoms, equation (3) still applies with the same meaning except that a, β, and γ now stand for a single bond in each case. Equation (3) and its analogues for more electrons provide a useful basis for constructing approximate potential surfaces in general.

It is of interest to compare the molecular orbital calculation of energy with the results of equation (3), which is based on the method of bond eigenfunctions. The approximate calculated energy for the system taking the state of the separated atoms as nq, where n is the number of atoms, is for two hydrogen atoms $E = 2q + 2\beta$; for three hydrogen atoms on a line, two equispaced from a central atom, $E = 3q + 2.82\beta$; for three hydrogen atoms forming an equilateral triangle, $E = 3q + 3\beta$; and for four hydrogen atoms forming a square, $E = 4q + 4\beta$.

To make the results for the molecular orbital method agree with experiment, we must assume that the repulsion between nuclei and between electrons, which has so far been neglected, will act to change E for three atoms on a line from 2.82β to about 1.8β, with correspondingly bigger corrections for a triangle and a square. Qualitatively, the results would then seem sensible. A systematic application of the molecular orbital method to activated complexes should be carried out. A beginning has been made by Sherman and Van Vleck and more recently by Pearson.

The statistical theory for reaction rates takes the form

$$= (A)(B) \ . \ . \ . \ K^{\ddagger} \frac{kT}{h} K \qquad (4)$$

where (A), (B), etc., are the concentration of reactants. $K^{\ddagger}$ is the equilibrium constant between the normal and activated state and is readily calculable in terms of partition function when the quantum mechanical calculation of barrier shape has been carried through; $\frac{kT}{h}$ is the frequency at the absolute temperature T. At room temperature, it has the value 5.6×10^{12}. The transmission coefficient K is calculable from quantum mechanics when the barrier shape is known, but can usually be taken as unity.

The specific reaction rate constant k' can thus be written as

$$k' \equiv K \frac{kT}{h} K^{\ddagger} = K \frac{kT}{h} e^{-\frac{\Delta F^{\ddagger}}{RT}}$$

$$= K \frac{kT}{h} e^{-\frac{\Delta H^{\ddagger}}{RT}} e^{-\frac{\Delta S^{\ddagger}}{R}}$$

$$= K \frac{kT}{h} e^{-\frac{\Delta E^{\ddagger} + p\Delta V^{\ddagger} - T\Delta S^{\ddagger}}{RT}} \qquad (5).$$

$$= K \frac{kT}{h} e^{-\frac{\Delta F_o^{\ddagger} + \int \Delta V^{\ddagger} dp}{RT}}$$

$$\frac{kT}{h} e^{-\frac{\Delta F_o^{\ddagger} + \overline{\Delta V^{\ddagger}} p}{RT}}$$

Here $\Delta F^{\ddagger}$ is the Gibbs free energy of activation at pressure p, and $\Delta Fo^{\ddagger}$ is this value at $p = o$. $\Delta H^{\ddagger}$ is the heat of activation; $\Delta S^{\ddagger}$ is the entropy of activation; $\Delta V^{\ddagger}$ is the increase in volume between the normal and the activated state. This equilibrium theory is applicable to all rate processes, both physical and chemical, where the activated state does not involve excessive barrier leakage. Where there is leakage, the corresponding contribution can be added to the contribution (5) for passing over the barrier. The only serious difficulty in estimating reaction rates a priori is the estimation of $\Delta E^{\ddagger}$.

a) Association reactions involving the breaking of no bonds ordinarily have $\Delta E^{\ddagger} = 0$.

b) Substitution reactions of the type $A + B - C \rightarrow A - B + C$ have $\Delta E^{\ddagger}$ equal to 5 percent of the

bond $B-C$ if A is an atom and if the bond $A-B$ is stronger than $B-C$. This defines the activation energy for the reverse reaction since it must be greater by the energy of reaction.

c) The reaction $A-B+C-D \rightarrow A-C+B-D$ has a $\Delta E^{\ddagger}$ about 28 percent of the two bonds broken if it is proceeding in the exothermal direction. The reaction $A-B+C-D \rightarrow A-D+B-C$ is likewise possible. Presumably that process will be most rapid which is most exothermal. This would be an interesting point to investigate.

d) In viscous flow, the free energy of activation is equal to the heat of vaporization of the flowing unit (usually a molecule) divided by 2.8. Diffusion very frequently proceeds by the same mechanism with the same activation-free energy.

e) In many organic reactions, the energy of activation is the energy of ionization of one of the reactants. Thus, if one nitrates solutions of benzene or of toluene in sulfuric acid, they go at the same rate, but if a benzene-toluene mixture is nitrated, the toluene is 29 times as apt to nitrate as benzene. This proves the critical complex does not contain benzene or toluene, but when formed it reacts with one or the other rather than decomposes spontaneously—reacting 29 times faster with the toluene molecule. This is in line with the usual situation in organic reactions. Thus, frequently, reactive complexes are formed which may react alternatively in a variety of ways.

f) Optically active synthesis, so common in biological systems, is certain proof of the existence of an optically active enzyme which acts as a templet, making better secondary bonds with the favored optically activated complex than with the other. This of course does not preclude simultaneous enzyme action on primary bonds. Such action, however, does not favor one optical isomer over the other.

g) Solvents with high dielectric constant, such as water, are such effective catalysts of intercombination between ionic crystals that there is no measurable slowness of solution and reaction.

THERE are many other general influences affecting individual reaction rates, but we turn now to another type of question. As we consider the natural rate processes occurring around us, they usually turn out to be a complex of the elementary processes for which the general theory has been outlined. This complex of elementary processes can often be thought of as being in fact a machine. We consider as a first example, very briefly, the process of growing old. Children from young or from old parents do not necessarily reveal the difference in the age of their parents. It thus is not individual cells that are growing old. Actually, in a certain sense, each individual is as old as the race; and, under favorable circumstances, the race might continue forever. Each individual is a sort of backwater along the stream of life. In the individual, certain cells have become specialized to form organs, and failure or unbalance between these organs sooner or later must spell disaster for the individual. This may be brought about by a series of elementary chemical processes or by physical damage. In any case, the interesting problem involves a whole complex of processes which will require reaction rate theory as a chief tool in its solution. Considerable progress has already been made in applying rate theory to living cells.

The weathering of a building or a rock on a mountain is another complicated series of events. Here chemical processes of solution proceed along cracks, started by unequal heating and cooling, that are then enlarged by freezing and thawing. Again, we require our theory of elementary rate processes if we are to bring order out of chaos.

As a final somewhat complicated example, consider mountain building. Here, besides the wrinkling that is to be expected with the contraction accompanying solidification of a liquid, we require an explanation of why mountain building seems still to be continuing about as briskly as ever. Clearly, some heat engine is at work which requires a continuing supply of heat. We think we know that no chemical reactions could be supplying the amount of heat required, so we turn to radioactivity. The known radioactive decay in granite supplies roughly 5 calories/million years/gram. Since the specific heat is about 1/3 calorie/gram, this would mean a temperature rise of about 15 degrees/million years at depths where the heat loss by conduction is small. In the course of time, such rocks must melt and so expand about 10 percent with an almost irresistible force. Thus, either the overburden will move up or strata will be pushed sideways or invaded, or liquid will pour out onto the surface, whichever process is easiest. With the melting of sufficient rock, heat losses are stepped up owing to convection; such losses are increased also by the foundering of part of the overburden and by the liquid finding its way to the surface. These processes, when they get in full swing, will cool the mass faster than heat is supplied, and a period of solidification and subsidence sets in, only to be followed in time by remelting. Thus, we have almost endless cycles of uplift and subsidence wherever the radioactive "yeast" is present. Especially is this true in the deltas formed at the margins of

continents, which in due course rise to make mountainous continental margins. Thus one confidently anticipates that a great mountain chain will eventually rise from the sediments now depositing at the mouth of the Mississippi.

In the earth, radioactivity is concentrated in the acid crust, which forms the continents. This concentration may be due in part to chemical affinity. However, since radioactive material has the property of becoming heated, its surroundings as a result automatically become lighter and so move upward, to be finally captured with other light materials in the surface crust. Explosions such as those at Krakatoa are the natural result of overheating large volumes of gases, such as water, and the distillates from partially oxidized organic materials, such as CO_2, NH_3, and the hydrocarbons. As radioactivity slowly heats buried sediments, oil distilled from buried organic matter moves upward along porous strata, dissolving material as it goes, to be trapped in appropriately situated domes. In other places where the temperature gets high enough, as around the edges of batholiths, natural smelting occurs and ore deposits form. Joly, especially, has emphasized the role of radioactivity and isostasy in geology. Although his views have been criticized and although the criticisms are probably justified in part, still radioactivity must be the chief fuel in the engine warping the surface of the earth.

Finally, we come to the dilemma that challenges all who would understand the broader aspects of reaction rate theory. The second law of thermodynamics tells us that the entropy of the world always increases, and the result of reactions is to move ever closer to the final equilibrium. In this "heat death," life and all the changes that interest us will have ceased except for possible fluctuations from equilibrium. Reaction rate theory is a theory of the possible fluctuations of reacting molecules from equilibrium. A reaction occurs at a moderate rate if the activated state must acquire 40 times the average energy of an oscillator, as a fluctuation at equilibrium; but if it must acquire 2 or 3 times this energy, one must wait geologic ages for the fluctuation to happen. It thus becomes fantastically improbable to think of a hot sun and a relatively cool earth ever originating by simple fluctuations from a region at constant temperature.

On the other hand, if the expanding universe because of inertia expands too far, only then to reverse itself and contract too far, we have in the expanded state a system with great randomness and relatively low energy density, reverting after some billions of years to a state of great material and energy density, which will of necessity be a state also of great order. During the expanding phase spontaneous processes tend to decrease the order and increase the entropy, whereas during contraction the reverse will be true. In this view, the second law of thermodynamics is a consequence of living in the expanding phase of the cycle. During the contracting phase matter and energy seem to be pouring in toward the center from outer space, spontaneously assuming states of greater order. In this way impossible fluctuations become the comparatively orderly consequences of a cyclic process. In any case, we are much further from a complete understanding of how systems out of equilibrium arise than we are from understanding how they subsequently proceed toward equilibrium.

A PLANT PHYSIOLOGIST LOOKS AT THE CANCER PROBLEM

PHILIP R. WHITE

Dr. White, of The Institute for Cancer Research, Philadelphia, received the Annual Thousand Dollar AAAS Prize in 1937.

IT IS a tragic platitude, one which, because it is a platitude, has remained too long beneath the surface of the public consciousness, that cancer destroys more human lives than any other disease except "heart failure." The heart maladies are mostly degenerative diseases. We will conquer them when we understand the nature of aging. Cancer, on the other hand, is par excellence *the* regenerative malady. I have deliberately refrained from calling it a "disease" because to most of us disease represents weakness, degeneration, breakdown. Cancer is primarily none of these but, rather, a lusty, undisciplined growth. Degeneration and weakness are only secondary sequelae thereto.

To many medical men, "cancer" is synonymous with "carcinoma," hence an infiltrating, metastasing, malignant growth of epithelial origin. The sarcomas, osteomas, etc., are excluded from this narrow category even when malignant. To the biologist and to most patients this is too narrow. A man dying of a malignant osteoma does not care whether it is of epithelial origin; it is to him a cancer. I am not a medical man, but a plant physiologist. In bringing me into the field of cancer research, it is tacitly understood that I will look at the problems from a plant physiologist's point of view. As a plant physiologist I take a broader view, and define cancer as Webster does, as "any malignant tumor." Malignant is "tending to produce death." A tumor is "any noninflammatory mass of tissue which is independent and unrestrained in growth and structure, without normal physiologic function, and arising without obvious cause from pre-existing tissue."

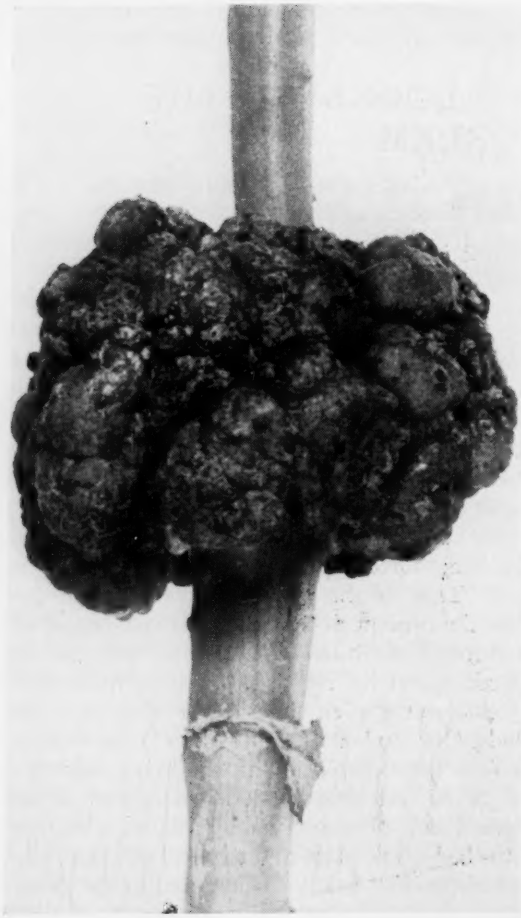
If one examines this definition carefully, it will be seen that, stripped to its essentials, the important words are "independent, unrestrained, unphysiological, and noninflammatory." Nowhere is there any mention of the sort of organism involved, or the tissue, or the type of cell. It might equally well be ectoderm or mesoderm, bone or nerve or skin. There is nothing to indicate if it is mammal or invertebrate or plant. The entire definition rests upon a characterization of the malignant cell. These adjectives "independent, unrestrained, unphysiologic, noninflammatory" set the malignant cell off from normal cells but do nothing more. If this is a sound definition, then cancer is an aberration of cells, no more, no less, and anyone who studies these aberrant cells is, per se, an investigator of cancer. It is through this gate that a plant physiologist may enter.

One fact has been brought home to me with increasing force repeatedly during the past few years. That is the unexpected realization that, while the concept of the cell as an *anatomical* unit has become an unconscious part of our thinking, the concept of the cell as a *physiological* unit has not penetrated even the surface of most of our minds. Our students (and teachers!) have clothed Hooke's dead chamber with the living sarcode of Du Jardin, yet have left it no more than a well-papered and -furnished room in the vast tenement of the body. The place of the cell as an *independent unit of function* has not penetrated to the general consciousness of scientists in spite of the clarity of the concepts formulated more than a century ago by Dutrochet and Schwann. I am sure that the first reaction of many readers will be to deny this, but I am equally sure that upon further consideration they will agree. In fact, if I interpret the written word rightly, such authorities as Conklin in 1940 (*Cell and Protoplasm Concepts,* 10th Symposium of the AAAS, #14, 1940) and Whitman before him in 1893 (*The Inadequacy of the Cell Theory of Development,* 1893) deny the correctness of that concept.

Yet it seems to me that it must be a sound concept, as anyone must testify who has had the experience of watching the movement of the connective tissue cells, the migration of nerve fibrils, even the spread of an epithelial membrane in tissue culture. I think that nowhere else is the complete functional independence of the cells which make up our bodies so clearly shown. In fact, as one watches the fibroblasts and macrophages moving hither and thither across the screen in a time-lapse movie of a tissue culture, one is reminded of two things; a swarm of bees as it is about to leave the

individual cells which carry on the process. The tissues and organs of which an organism is constructed are merely means of integrating the varied activities of the constituent cells into a machine that can function. True, this integration is essential for the normal functioning of the organism as a whole, but it is not essential for the normal functioning of the individual parts, the cells.

It seems strange that this fact has not really penetrated below the surface of our thinking. We still think either in terms of heart action, kidney function, muscle contraction, blood circulation—that is, in terms of *organ* function—or, at the other extreme, of pancreatic juice digestion, visual purple, cytochrome, insulin—that is, in terms of chemical processes outside the cell. In none of these are we thinking in terms of the cell itself. Disfunction as we usually encounter it, except in cancer, is probably more often a matter of failure of the *integrating* mechanisms than it is of failure of the cells. In fact, the most ubiquitous example of integrative failure, aging and death, is not a failure at the cellular level, as the unlimited survival of

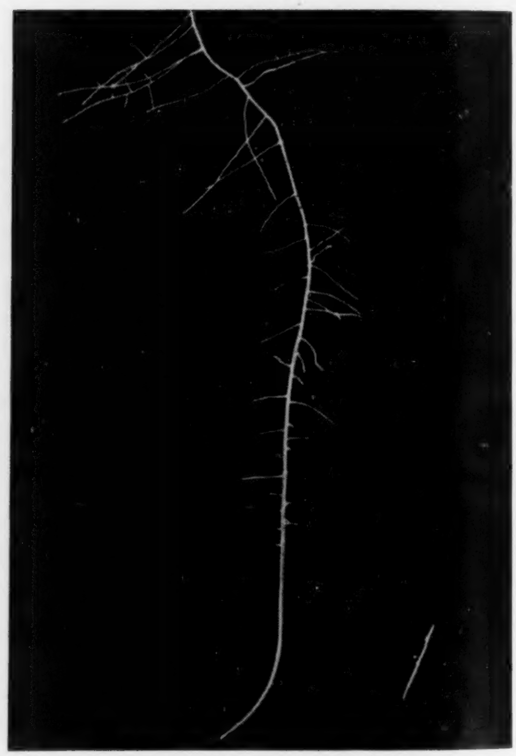


Crown gall on sunflower plant, resulting from a single needle-prick inoculation with a broth culture of *Phytomonas tumefaciens.*

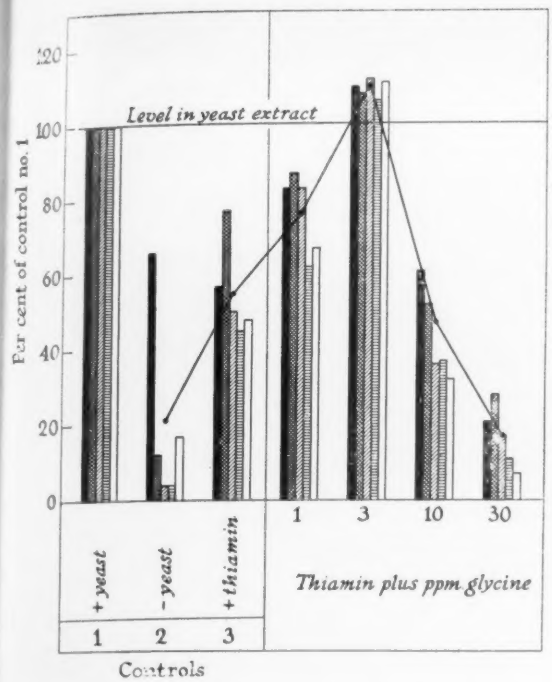
old hive, and the classic pictures of the molecules in a gas, or, perhaps better still, the electrons in a solid. One recalls Schwann's words:

> That not every cell, when separated from the organism, *does*, in fact, grow (when so separated) is no more an argument against this theory [of physiological independence] than is the fact that a bee soon dies when separated from its swarm a valid argument against the individual life of the bee.

One might add that the successful cultivation of cells outside the organism, in tissue cultures, removes even this argument. The cell *is* a physiological unit. When a gland secretes pituitrin it is the *cell* that is carrying on the physiological process, not the gland; when a nerve carries a stimulus it is the *nerve cell* that performs the conduction, not the complex system which we call a nerve; when a muscle contracts it is the individual cells which carry out the physicochemical changes responsible for contraction; when a tissue respires it is the



Tomato root tips cultivated for 1 year on a nutrient chemically defined except for inclusion of an extract of 100 mg of yeast per liter. This unknown was later replaced by synthetic materials. *Right*, a fragment about 1 cm long, as introduced into the nutrient. *Left*, culture after 12 days' growth. (From White, P. R. *Plant Phys.*, 1934, 9.)



Histogram showing effect on growth of excised tomato roots of substitution of thiamin and of thiamin plus glycine for the yeast extract. Roots were subsequently grown in this thiamin-glycine nutrient for 4 years without diminution of growth rate, before its substitution by a slightly more complex defined nutrient. (From White, P. R. *Plant Phys.*, 1939, **14.**)

tissue cultures has clearly shown, but a failure at the integrative, organismal level.

I have said that we do not yet think in terms of the cell as an independent physiological unit. Perhaps this is not important in dealing with generalized diseases such as arthritis or pernicious anemia, nor even with specific organ ailments such as kidney disease or heart failure, but in diseases of a specific type of cell, irrespective of its location, such as leukemia, multiple sclerosis, or Hodgkin's disease, it is of very great importance. It is imperative that we overcome this inhibition in our thinking.

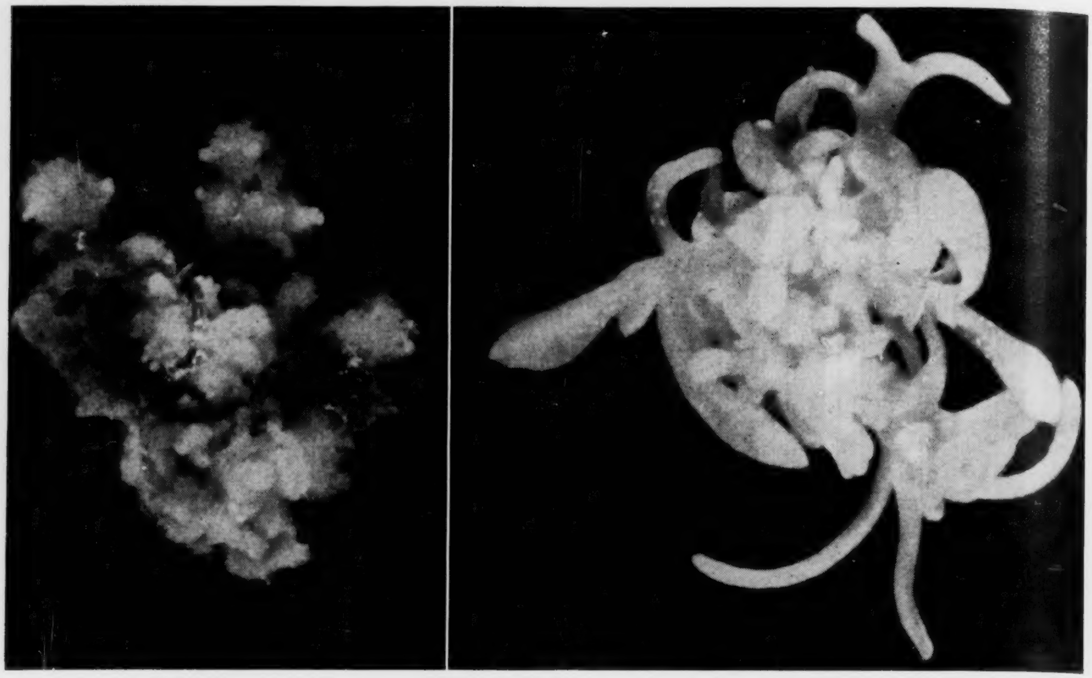
But it is not quite enough that we think of cells merely as physiological units, for if we do that we may be satisfied to study their processes in unicellular organisms, protozoa, bacteria, etc., as indeed most "cellular physiologists" have done. Study of the cell as a physiological unit will be medically useful only if we never lose sight of the fact that what we are really interested in is its function as a unit integrated into a multicellular organism. This I take to be Conklin's real reason for emphasizing the organismal viewpoint. We isolate the cell not so much because we are interested in its unitary behavior per se as because that is the only means by which we can distinguish between those facets of its behavior that are integral to itself, and consequently cannot be modified, and those which are imposed by its surroundings, and therefore *can* be modified, potentially to the benefit of the organism.

This is, therefore, the philosophy of the tissue culturist, a philosophy which permits a plant physiologist to study cancer.

One of the grave disappointments of the last generation has been the relatively minor contributions that tissue culture has made to our understanding of cancer. At the time the technique was first being developed in the early 1910s, it seemed to hold such very great promise. It was early discovered that cancer cells would usually liquefy plasma more rapidly than normal cells, but it soon became evident that epithelium in general tends to liquefy plasma. Lipman and others verified with tissue cultures what had already been determined on tissue slices, that tumor cells have a somewhat different level of anaerobic glycolysis. The higher sensitivity of cancer cells to X-rays, long known from clinical observations, was verified in tissue cultures by Strangeways and others. These were not new facts. Perhaps the most important contribution that tissue culture has made so far is the apparent demonstration by Gey, and perhaps by Earle (depending on one's interpretation of the results), that normal cells can be spontaneously changed into cancer cells without apparent cause. This last observation has not been verified by others and is open to alternative explanations. It represents no more than a proof (if it be one) of something we have long suspected of being true, but it does give us a means of studying the phenomenon and ultimately of unraveling the tangled web of causation behind it.

All this may seem a bit remote from plant physiology in general and water movement in plants in particular. The remoteness is not an illusion, but it is a matter of history. My studies in water movement, while a natural outcome of certain phases of my root studies, which were themselves a means toward my more basic goals of tissue nutrition, were nevertheless a *divertissement*, a temporary excursion into green pastures walled off from my main field. That main field is and always has been the study of cancer.

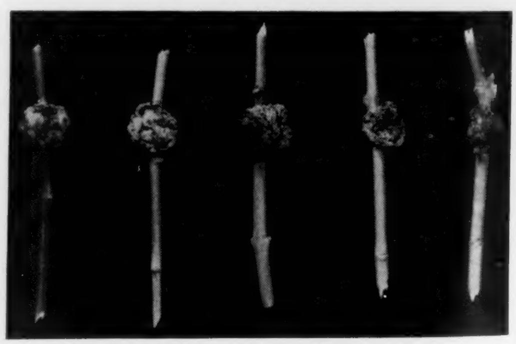
My own first approach to the fields of both cancer and of tissue culture was made a little more than twenty years ago. "Crown gall" had long been known as a "plant cancer." Twenty



Two cultures of a hereditary tumor from the hybrid *Nicotiana langsdorffii* × *N. glauca*. That at the left was grown undivided for 20 weeks on a nutrient made semisolid by addition of agar. It has remained undifferentiated. That at the right was kept for 10 weeks on the agar nutrient and then transferred for 10 weeks to a nutrient identical except for the omission of the agar, resulting in the culture being immersed in the nutrient fluid. Under these conditions it has formed highly organized stems and leaves. (From White, P. R. *Bull. Torrey Bot. Club,* 1939, **66**.)

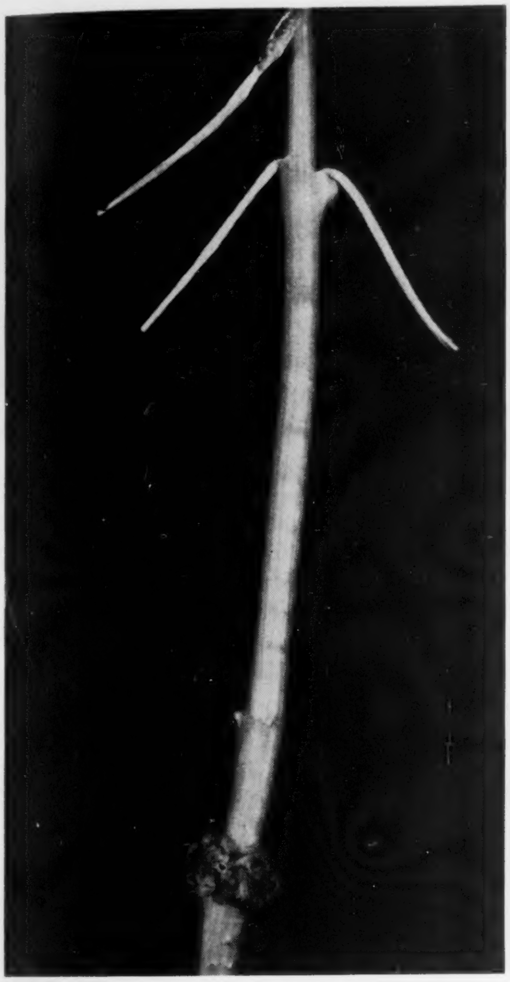
years ago the desirability of growing crown-gall tissue in vitro for more accurate control and closer study was already well recognized. The possibility of accomplishing such cultures seemed remote because of the presumed universal presence of crown-gall bacteria in the tumors. If the phenomenon of tumor inception in plant tissues was to be studied in detail, it seemed imperative to devise methods of growing not tumor tissues but normal tissues. These could then be treated, perhaps with bacterial products, to transform them into tumor tissues in vitro without the cultures being overrun with bacteria. Such was the reasoning. Methods for growing such normal plant tissues were not available at that time, and the task of developing these methods fell to me.

The cultivation of plant tissues proved difficult, slow to master, but not impossible. Past history had shown many of the things which could not be done with facility. It was clear that the basic problems were nutritional ones, and that these problems would be most easily solved by study of rapid-growing, perennially meristematic organs, of which the root tip presented a prime example. It was for that reason that preliminary work was concentrated not on crown gall itself, nor even on tissues commonly subject to crown-gall infection, but on sound, healthy root tips. The preliminary goal was essentially reached in 1939 by the perfection of synthetic nutrients for excised root tips by Robbins, by Bonner, and by myself, and the simultaneous extension of the use of nutrients of this sort to the cultivation of less organized tissues by Gautheret, by Nobécourt, and by myself. Nobé-



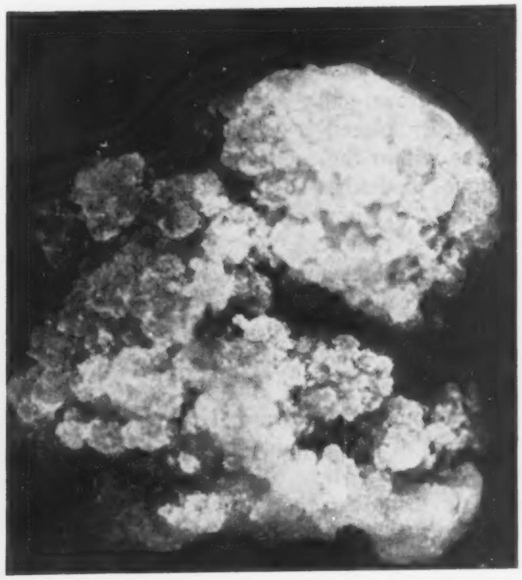
Stems of *Vinca rosea* into which were grafted fragments of tissue derived originally from crown-gall tumors rendered bacteria-free by fever therapy (Braun) and grown for 1 year as tissue cultures. Massive sterile tumors result from such grafts. (From White, P. R. *Amer. J. Bot.* 1945. **32**.)

court, like myself, was led to investigate the question originally by an interest in crown gall. Gautheret has been led by somewhat different paths to make very important contributions to the crown-gall question. Subsequent developments have all rested on the variety of procedures published in 1939.



A sunflower plant inoculated with crown-gall bacteria just above the cotyledons when a seedling. Secondary tumors have appeared at various points above the site of inoculation. These are typically free of crown-gall bacteria. From them bacteria-free tumor tissue cultures can be isolated. (From White, P. R., and Braun, A. C. *Cancer Research*. 1942, **2**.)

It was toward the end of this preparatory decade that I jumped the fence outlining my original problem to examine briefly a quite unrelated phenomenon, the secretory capacity of excised roots under in vitro conditions. It was for this unplanned departure from the straight and narrow



A culture of carrot maintained in vitro for 6½ years. Cultures of this strain when "habituated" by cultivation on an idoleacetic acid medium and then transferred to an IAA-free medium take on properties closely resembling those of tumor tissues. (From Gautheret, R. J. 6th Growth Symposium, 1947.)

path that I unexpectedly garnered the award (should I say "the wages of sin"?) that is responsible for my writing this present article. I have not returned to that subject since. I might have answered your editor's request by writing a paper on "Studies on Water Movement in Plants Which Have Not Been Made in the Past Decade." I cherish the delusion that what I am writing is more important.

Science often progresses by a happy concatination of seemingly unrelated events. At just about the time our technique for cultivation of normal tissues, on which we had spent ten years, was developed to a point of real usefulness, an unexpected break occurred in our favor. In 1941, Armin Braun rediscovered the bacteria-free secondary tumors which arise at a distance from the primary focus of infection in cases of crown gall of certain plants in the family Compositae. These had been described by Erwin F. Smith a quarter of a century earlier, but erroneously interpreted. This discovery immediately placed at our disposal bacteria-free crown-gall tissue, of what soon proved to be considerable variety, for in vitro study in tissue cultures without the expected necessity for going through the intermediate stage of working with normal tissues. Followed quickly by Braun's studies on thermal behavior of crown gall (likewise an expansion of earlier incomplete and neglected

studies by Riker) and by De Ropp's redemonstration (again an observation of Smith's, misinterpreted by him) of the bacteria-free nature of many primary crown-gall tissues, these investigations unexpectedly placed a whole range of crown-gall tissues within the scope of our recently acquired culture methods. We thus now have at our command (1) normal tissues which can be caused to become tumorous under in vitro conditions. (This Gautheret appears to have accomplished in what he calls "habituation" brought about by treatment with naphthalene acetic acid, a substance analogues of which can be obtained from crown-gall bacteria.) These can be used as bases of comparison with crown-gall tumor cultures. We also have at our command (2) tissues which are tumorous at the time of isolation (crown-gall secondaries) and can therefore serve for comparison with normal tissues, for comparison with tissues made tumorous by in vitro treatment, and, last, for treatment in vitro with procedures which we may hope will give us the converse, "curing" a tumorous tissue, rendering it again normal in behavior.

We are only just beginning this phase of our studies. It would be a mistake to suppose that we have yet even approximated these goals. There are endless details to be worked out. We may even hope for entirely unexpected leads in the future. Nevertheless I think we have shown that since cancer is an aberration of cells, and since this sort of aberration can and does occur in many different sorts of organisms, including plants, plant physiologists have a not inconsiderable contribution to make to the study of cancer. Although an AAAS award for a paper on "Root Pressure, an Unappreciated Force in Sap Movement" may seem a strange basis on which to build a career in cancer research, there can be no doubt that the heartening effect of such a symbol of approbation from one's peers does contribute mightily toward the maintenance of productive energy, interest, and accomplishment.

PHOTOGRAPHIC EXHIBITIONS

Entries in the *Second Annual International Photography-in-Science Salon* will be received by The Scientific Monthly July 26–August 16, 1948, inclusive. They will be judged on August 21, and those accepted will be shown in the Natural History Building, U. S. National Museum, Washington, September 1–21. Already booked through April 1949, showings after that date may be arranged.

The Biological Photographic Association will hold an exhibition in Philadelphia, September 8–10, in Houston Hall, University of Pennsylvania, at which prints, color transparencies, and motion pictures in the field of biological photography will be shown. There will also be organized symposia and demonstrations.

PROTOPLASMIC CONTRACTILITY

PRESSURE EXPERIMENTS ON THE MOTILITY OF LIVING CELLS

DOUGLAS MARSLAND*

Professor Marsland, Department of Biology, New York University, shared the Annual Thousand Dollar AAAS Prize in 1941 with Dr. Dugald E. S. Brown, director of the Bermuda Biological Station for Research, and Professor Frank H. Johnson, of Princeton University, whose article also appears in this issue.

THE pressure of our atmosphere, which bears directly down upon all land-dwelling animals and plants, does not impose a very heavy burden. At sea level, in fact, this pressure amounts to only 14.7 pounds per square inch; and even if an animal migrates from the highest mountain to the deepest cave, the pressure changes are rather small. Such small variations are important indirectly, because they drastically alter the quantity of oxygen and other gases that penetrate into the protoplasm and body fluids. But the direct effects of pressure upon the vital processes of land-dwelling organisms are almost negligible.

The deep sea, however, imposes a different set of conditions. The cumulative weight of the heavy overlying water increases with the depth, and the pressure keeps mounting, roughly at the rate of one pound for every two feet below the surface level. At a depth of three miles, where a wide variety of species are known to live (Fig. 1), the pressure has climbed to almost 8,000 lbs./in^2. Such pressures so drastically modify the vital activities of an organism that truly deep-sea forms cannot survive at surface pressure, and, conversely, a surface form is likely to die if suddenly exposed to deep-sea conditions. Accordingly, we are faced with the problem of understanding how pressure affects the fundamental life processes of the protoplasm. This problem is important because life originated in the aquatic environment, and pressure has played a significant role in determining the evolution of aquatic species.

It is of critical importance in cell physiology to acquire an understanding of the various chemical and physical reactions which go on continually in the living protoplasm. These metabolic reactions provide energy without which the vital activities of the animal must come to a halt. The physiologist continues, therefore, to expose cells to many different experimental conditions. He uses high and low temperatures, much and little oxygen, or all manner of toxic and nontoxic compounds—knowing that the resulting changes in the behavior of the cells are bound to reveal something about the fundamental chemical and physical processes of the protoplasm. The influence of high pressure upon the activities of living matter has not received an adequate share of attention, although pressure experimentation has gradually gained momentum since 1880. This modern period in the field of pressure physiology began with the important deep-sea dredging expedition of the *Talisman*, which proved that many organisms live at depths exceeding eight miles and at pressures approaching 15,000 lbs./in^2.

The protoplasmic reactions, of course, are numerous and very complex. A starting point can be reached, however, by considering how pressure may influence any given physical or chemical reaction without reference to where it may occur. If we take, for example, the freezing of water into ice, at a temperature of 0° C and at atmospheric pressure, the water-ice system is at equilibrium. Under these conditions freezing and thawing are exactly balanced so that no net change takes place; but, if the pressure is raised, the ice will melt. In other words, pressure is capable of shifting the equilibrium of the water-ice system, and it has been found that pressure exerts a similar effect upon reversible reactions in the protoplasmic system.

An analysis of the action of pressure upon physical and chemical reactions leads to the general conclusion that volume changes are of critical importance. Pressure invariably opposes any change that increases the volume of the system, while simultaneously it favors any change that decreases the volume. In the water-ice equilibrium, for example, water expands as it freezes and pressure opposes this change, whereas thawing involves shrinkage of volume, which is favored by the pressure. Moreover, since volume changes occur in a majority of physical and chemical reactions regardless of whether these occur in living or in nonliving

* The author wishes to make grateful acknowledgment to Dr. Dugald E. S. Brown, director of the Bermuda Biological Station for Research, who collaborated directly in the work on amoeboid movement and in the studies on the equilibria of various inanimate gels, and who has been a generous counselor throughout all the other work.

Fig. 1. Remarkable deep-sea photograph showing bottom of the ocean at a depth exceeding 1 mile (2,120 meters). Note large unidentified fish and the numerous tracks and castings in the sand. This and other deep-sea photographs taken by the staff of the Woods Hole Oceanographic Institution under the leadership of Maurice Ewing indicate that a wide variety of species inhabit the ocean depths.

matter, it is not surprising to find that the metabolic reactions of living cells are very sensitive to changes in the environmental pressure; this explains why the visible activities of cells are so drastically modified according to pressure conditions.

In the protoplasm one important reaction, the sol-gel reaction, goes on more or less continuously. For many years physiologists have noted that protoplasm keeps changing back and forth from the sol state, which is almost as fluid as water, to the gel state, which displays a consistency like firmly set gelatin. The functional significance of these protoplasmic gelations was not understood very clearly, however, until quite recently, and the experiments in which living cells have been exposed to high pressure have been very useful in clarifying this classic problem. It was soon found that the gelling of the protoplasm, like the freezing of ice, is drastically inhibited when the pressure is raised. When firmly gelled, apparently the protoplasm possesses contractile powers that enable the cell to change shape and display motility. And, conversely, when the protoplasm is liquefied, as by the solling action of high pressure, contractility is lost and the cell becomes immobile. This, in short, is a general conclusion that has emerged from the pressure experiments. But since our purpose is to describe the results in more detail, main attention will be given to amoeboid movement and to the movements of dividing cells, with a brief consideration of the motility of certain pigment cells.

Amoeboid movement. Perhaps the most primitive form of locomotion is amoeboid movement, which can be observed directly in *Amoeba* and related one-celled animals. This movement is not restricted to the locomotion of such lowly forms, however. It is displayed by many cells in the human body, such as the white blood cells—which leave the capillary vessels in search of infective bacteria—and many connective tissue cells, which also are free to wander through the tissue spaces. Moreover, in the embryo, before the tissues assume their adult form, many embryonic cells employ amoeboid movement as they migrate to their proper stations in the body, where final differentiation will occur.

The fascinating simplicity of amoeboid movement can be observed to best advantage when a microscope is brought to bear upon a large *Amoeba* in which only a single pseudopodium is being formed (Fig. 2). Here one sees the smooth flowing of the protoplasm, which carries the nucleus, vacuoles, and granules forward through a central channel of the cell, in the direction of the advancing pseudopodium. It is seen also that the superficial part of the protoplasm just under the surface

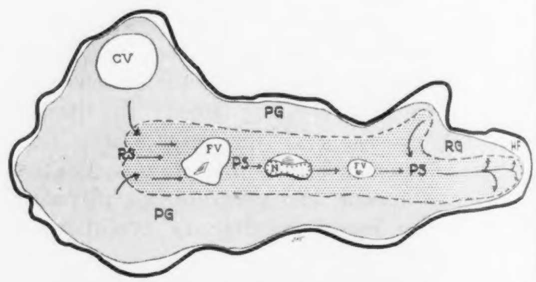


Fig. 2. Diagram of *Amoeba* showing the mechanism of amoeboid movement. Forward-streaming of the fluid protoplasm, or plasmasol (*PS*), is caused by a contractile force exerted by a surrounding layer of gelated protoplasm, the plasmagel (*PG*). This streaming carries the nucleus (*N*), food vacuoles (*FV*), and other granules out into the actively extending pseudopodium (*PS*). New gel keeps forming at *RG*, reinforcing the wall of the pseudopodium as it lengthens, and the old gel at the other end (*RS*) of the *Amoeba* keeps undergoing solation, forming new plasmasol. Other structures shown are the contractile vacuole (*CV*), which serves to pump excess water from the cell and the hyaline fluid (*HF*), which exudes through the contracting plasmagel, especially at ends of the pseudopodia.

membrane of the cell does not participate in the central streaming; rather, it maintains a fixed position and seems to form a wall that guides the flowing protoplasm outward through a definite channel. It is thus clear from direct observation that the protoplasm of the *Amoeba* is differentiated into two parts: the plasmasol, the fluid, deep-lying part, which flows while the *Amoeba* is moving; and the plasmagel, the firmly gelated surface part which encases the plasmasol. A cycle of sol-gel changes is necessary if the amoeboid movement is to continue. New gel is added to the walls of the plasmagel tube just behind the advancing tip of the pseudopodium (Fig. 2), and the old gel at the opposite end of the cell is continually transforming into new plasmasol, which joins the forward stream. The flowing of the plasmasol appears to originate from a contractile force exerted by the surrounding tube of plasmagel. However, prior to the pressure studies this "contractile hypothesis" was supported by very little experimental evidence.

The problem of observing the *Amoeba* while it is being subjected to high pressure requires a special chamber, made of stainless steel. Very strong glass (Herculite) must be used in the windows, since otherwise the thickness of the windows would not permit the cells in the chamber to lie within reach of the lens system of the microscope. Even so, it is necessary to use a special objective (Leitz, U.M. 4) which has an exceptionally long working reach; and since the cells tend to fall to the bottom of the chamber, an inverted microscope must be employed. Cells are observed at a magnification of 600 diameters. Various pumps may be used to build up the pressure in the chamber, although in most of the present experiments an automobile jack of the hydraulic type was modified to serve as a pump.

If we start with an actively flowing *Amoeba* in the chamber and observe what happens when the pressure is suddenly raised to a high level (8,000 lbs./in^2.), the effect is quick and dramatic. Immediately the elongate pseudopodium collapses and soon the whole cell rounds up into an immobile sphere. Apparently, pressure induces a drastic liquefaction of the plasmagel, and now the protoplasm behaves like any small droplet of fluid in which a rounded form results from tensional forces at the liquid surface.

While the pressure is maintained no sign of streaming can be detected in the rounded *Amoeba*, and gradually the nucleus and other visible particles in the protoplasm begin to fall into the lower half of the cell. As soon as the pressure is released, however, activity begins again. Within two minutes, one or more pseudopodia begin to thrust out from the cell, and very soon locomotion becomes very active again.

Not all parts of the plasmagel of the *Amoeba* are equally susceptible to the liquefying action of high pressure. In fact, the newly formed gel near the pseudopodial tip displays a distinctly weaker structure than the older gel in and near the body of the cell. Such a difference shows very plainly when more moderate pressures are applied to specimens with very long slender pseudopodia—a form assumed by *Amoebae* when placed in distilled water rather than in natural pond water. When the pressure approaches 6,500 lbs./in^2., the first collapse and rounding of the protoplasm involves only the extremity of each pseudopodium (Fig. 3). Even at 6,500 pounds, however, the whole cell gradually becomes rounded and immobile unless the high

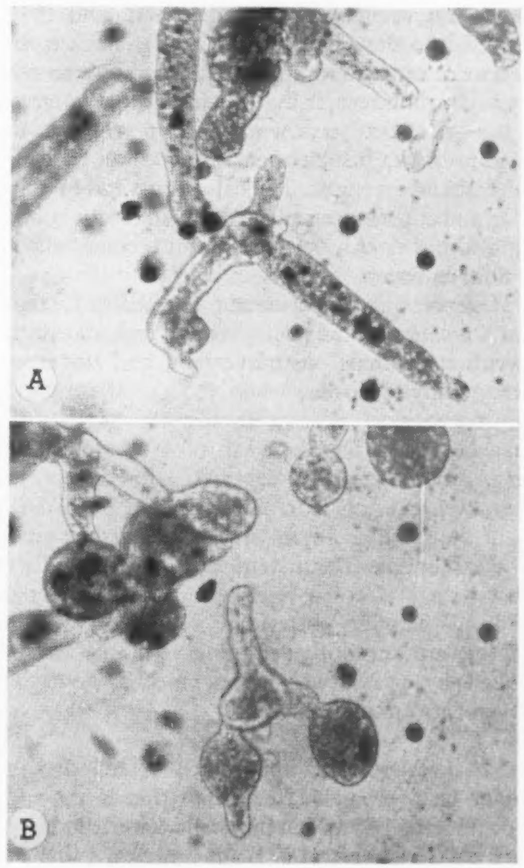


Fig. 3. Liquefaction and collapse of the ends of the pseudopodia of the *Amoeba*, which occurs when pressure is suddenly raised to a moderately high level. *B* was taken 1 second after pressure was raised from 1,500 (*A*) to 6,500 lbs./in^2. Such bulbous pseudopodia are transient, however, since the whole cell soon rounds into an immobile sphere if the pressure is maintained. At 6,500 lbs./in^2. the strength of the plasmagel is reduced to less than 20 percent of the atmospheric value.

pressure is released within a minute or two. Thus the newly formed gel near the advancing end of a pseudopodium, being relatively weak, disintegrates quickly even at moderate pressures, whereas the solation of the older gel is less drastic, and the rounding of the protoplasm occurs less rapidly.

Quantitative measurements of the pressure effect upon the gel strength in the *Amoeba* have also been made, and these data support the conclusion that movement is inhibited in proportion to the weakening of the plasmagel structure. For these experiments a special chamber was constructed in which the pressure could be maintained while simultaneously the *Amoebae* were being whirled in a centrifuge. Under centrifugal force the heavier elements in the protoplasm, such as the nucleus and visible granules, tend to be thrown into one end of the cell, whereas lighter components, such as the contractile vacuole and fat droplets, tend to be buoyed into the other end (Fig. 4). Such a displacement of particles through the protoplasm cannot occur, however, if the gel strength is too great. If the gel structure is weakened by pressure, the rapidity of the displacement provides an index of the residual strength. Thus it is possible to show that, under pressures adequate to induce a rounding of the *Amoeba,* the protoplasm becomes almost as fluid as water.

The pressure experiments demonstrate, then, that a gelation of the protoplasm is essential to the execution of amoeboid movement, and that complete paralysis ensues when the gel structure of the protoplasm is destroyed. The plasmagel of the *Amoeba,* which has the form of a tube enclosing the plasmasol, exerts a contractile force upon the plasmasol, causing it to flow outward toward the tip of the pseudopodium where the gel strength is at a minimum. To sustain the movement there must be a cycle of sol-gel changes whereby new sol keeps forming posteriorly and new gel keeps building up anteriorly; if these reactions are deranged by pressure or other means, amoeboid movement cannot occur.

Cell division. The importance of cell division cannot be overemphasized, since this is the only known method by which new cells come into being. Each cell of an animal or plant represents the current link in an uninterrupted chain of cell divisions which has extended back into ancient times and which may extend into the infinity of the future.

Cell division is best observed in the very early embryo when a single large cell, the fertilized egg, begins to divide (Fig. 5) and continues redividing until the many cells of the adult body have been

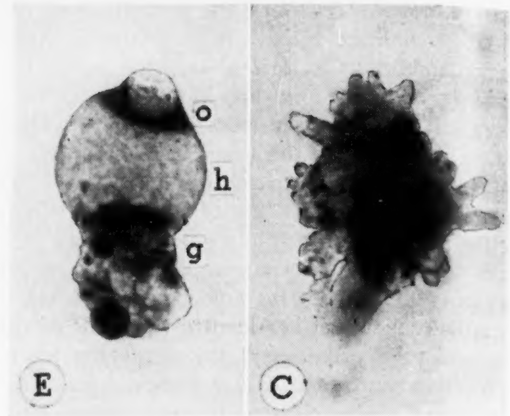


Fig. 4. Under high pressure the gelated protoplasm of the cell undergoes liquefaction and loses its contractile power. These two cells (*Amoebae*) were whirled simultaneously in the same centrifuge, but *E* was compressed at 8,000 lbs./in^2., whereas *C* was at atmospheric pressure. In *C* the protoplasmic gel is too stiff to allow any displacement of imbedded particles, but in *E* all the heavier granules (*g*) have been thrown into the lower part of the cell, whereas the lighter oil droplets (*o*) have been buoyed into the upper part, leaving a clear, or hyaline, zone (*h*) between. Note that the nucleus (large, dark, round body) is heavy and goes to the centrifugal end, whereas the contractile vacuole (round clear vesicle in the oil cap) is much lighter.

formed. In fact, these early cleavage divisions occur on a very precisely timed schedule. Given the exact temperature and the time at which the sperm are added to a batch of unfertilized eggs one can plan an experiment knowing precisely when each of the successive cleavage divisions will occur.

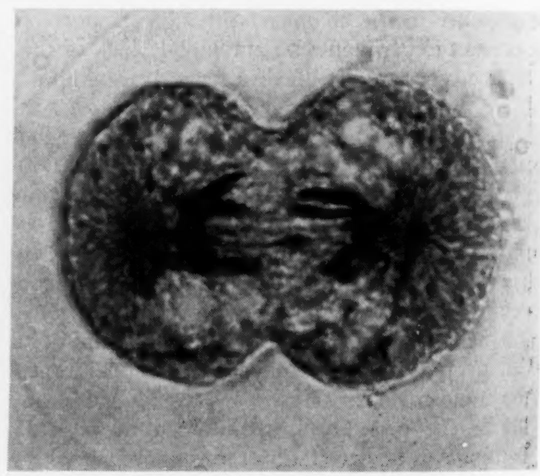


Fig. 5. Dividing egg cell (mag. × 1,360). This is the first of the many divisions by which the numerous cells of the developing embryo are to be formed. The chromosomes, spindle, asters, and other important structures may be identified by comparing this photograph with Figure 6. (Courtesy, General Biological Supply House.)

Subsequent to insemination, the sperm head, or male pronucleus, penetrates into the egg cell, migrates toward the center of the egg, and finally fuses with the nucleus of the egg (female pronucleus). Thus the nucleus of the fertilized egg is a duplex body, made up half and half of chromosomes of maternal and paternal origin. And, since each chromosome perpetuates itself throughout the successive cell divisions in the developing embryo, each of the parents has an equal potentiality in determining the heritable qualities of the new individual.

Focusing attention on the first cleavage division at the time when the cytoplasm of the egg cell is pinching itself into daughter halves, we find a configuration such as is shown in Figures 5 and 6.

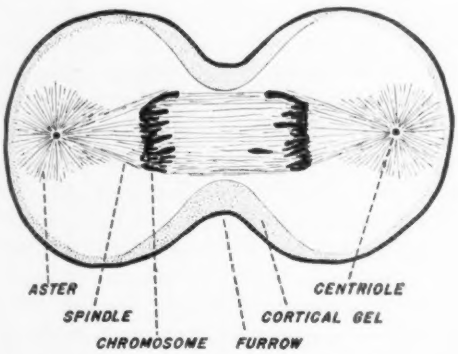


Fig. 6. Diagram of a dividing egg cell, showing important structures visible when the cell has been fixed and stained.

Already the chromosomes have divided and the daughter chromosomes have been moved toward the ends of the spindle. Also, the cleavage furrow has made good progress in the work of cutting through from the equator of egg toward the center of the spindle. These movements can be observed directly in the living egg, and an interesting problem is to find out how they are effected.

The egg cell is definitely comparable to the *Amoeba* in that the cytoplasm shows a similar differentiation into a gelated layer at the surface and a much more fluid solated portion occupying the central region of the cell. In the unfertilized egg, however, the plasmagel is not very firmly set. At this time it is quite easy to effect a displacement of pigment granules which are embedded in the cortical gel (as well as in the deeper sol) when the cells are placed in a relatively weak centrifugal field, such as 1,700 times gravity. But in the fertilized egg, especially when the time for furrowing approaches, the strength of the cortical gel undergoes an increase that is more than tenfold. Now a centrifugal field of 17,000 times gravity is not strong enough to budge the cortical granules (Fig. 7). Moreover, the gel strength of the cortex reaches a maximum in a bandlike region which encircles the equator of the cell where the cleavage furrow will soon appear. As one observes the dividing egg, this gelated cleavage girdle (heavy stippling in Fig. 6) appears to contract, constricting the cell like an hourglass and finally cutting the egg into daughter halves. This conclusion is well substantiated by experiments in which dividing cells are subjected to the solating effects of increased hydrostatic pressure.

A dividing egg in the pressure chamber at the time when the hourglass form is well developed can be observed while the pressure climbs. When the pressure reaches 5,000 lbs./in^2. or more, the progress of the furrow halts abruptly (Fig. 8). Then the furrow begins to recede. The recession is relatively slow at lower, but quite rapid at higher, pressures, and within 2–4 minutes the egg has reassumed its original spherical form. The retreat of the furrow reverses itself, however, as soon as the pressure is released (Fig 8). In fact, one can induce the furrow to advance and retreat alternately for several times by releasing the pressure after each compression. Finally, however, if the furrow is held back for more than 15 minutes, it will fail to cut through when the pressure is released. Then no refurrowing will occur until it

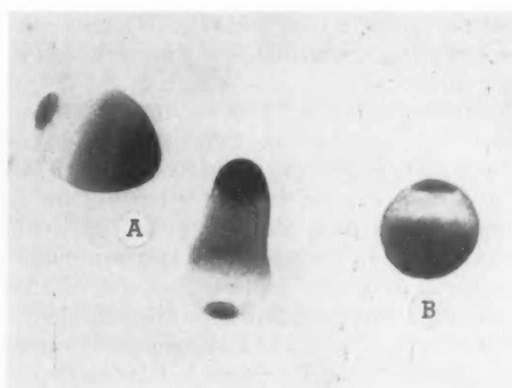


Fig. 7. A tenfold increase in the strength of the cortical gel of the egg cell occurs just before cell divides. All these cells were centrifuged simultaneously at the same high force (17,000 × gravity). In the unfertilized eggs (*A*) the structure of the cortical is weak, allowing the pigment granules to be thrown into a black, densely packed mass at the heavy end of the stretched cell and completely clearing the hyaline zone. But in the fertilized egg (*B*) 5 minutes before the furrow will appear, the pigment granules remain fixed in the strongly gelated cortex so that (*a*) the cortex of the hyaline zone remains granular, (*b*) a dense black pigment mass is *not* formed, and (*c*) the cell remains unstretched by the high centrifugal force.

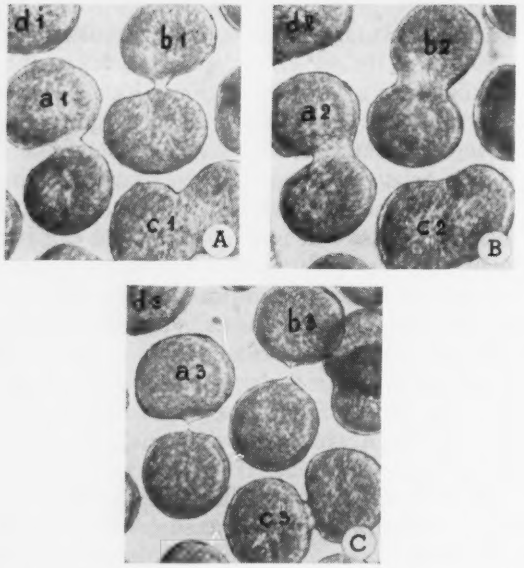


Fig. 8. The retreat of the cleavage furrow which occurs when a dividing egg is compressed at pressures above 5,000 lbs./in². In *A*, at atmospheric pressure, the advancing furrows have cut almost completely through, so that the daughter cells are connected by mere strands of protoplasm. Yet in *B*, 2 minutes after the pressure was raised to 6,500 lbs./in²., there was a very marked retreat, which, however, was reversed as soon as the pressure was released (*C*). Note particularly the series b_1, b_2 *and* b_3, which shows the furrow most clearly. The fertilization membranes of these eggs were removed to allow for a clearer view of the furrows.

is time for the second cleavage, whereupon a double furrowing usually occurs; thus three or four daughter cells are simultaneously formed from the single parent cell.

At pressures below 5,000 lbs./in²., the furrow does not recede, but the rate at which it cuts through the egg is definitely retarded. At the 4,000-lb. level, for example, it takes 9 minutes for the furrow to pass from the equator to the spindle center, instead of the normal atmospheric time of 3 minutes (at 20° C). In fact, the curve obtained by plotting this retardation as a function of pressure fits very closely when superimposed upon a plot of the cortical-gel strength in relation to pressure, as measured by the pressure-centrifuge technique at the time when cleavage is occurring (Fig. 9).

The simplest and most direct explanation of the pressure effects upon the various egg cells that have been studied is that cleavage results from the contraction of the equatorial girdle of gelated protoplasm. In the gelated state, apparently, the protoplasm possesses contractile properties lacking in the solated system. Therefore, the forces which enable the cell to change its form during both cleavage and amoeboid movement are fundamentally similar in nature.

That the spindle and asters of the dividing cell (Fig. 6) are also gel structures which are susceptible to dissolution under high pressure appears evident from the work of Daniel Pease. No trace of the spindle and asters can be found in egg cells which are killed and stained immediately after a short exposure to pressures of 5,000–6,000 lbs./in².; this level of pressure is adequate to abolish the displacement of the daughter chromosomes from the center toward the ends of the spindle area. If the cell is not killed directly after the pressure is released, several new asters begin to appear in the cytoplasm, and, if one of these asters happens to come into contact with the chromosomes, a peculiar half-spindle develops in the region between the chromosomes and the astral center. The fibers of the half-spindle become attached to some of the chromosomes, and it is only in the presence of such half-spindles that any movement of the chromosomes toward the astral center can be observed. Experiments by Pease substantiate the view that the "traction fibers" of the spindle are gel structures which effect a displacement of the chromosomes because of their contractility.

Motility of pigment cells. In the skin of many fishes and certain other animals one finds a multi-

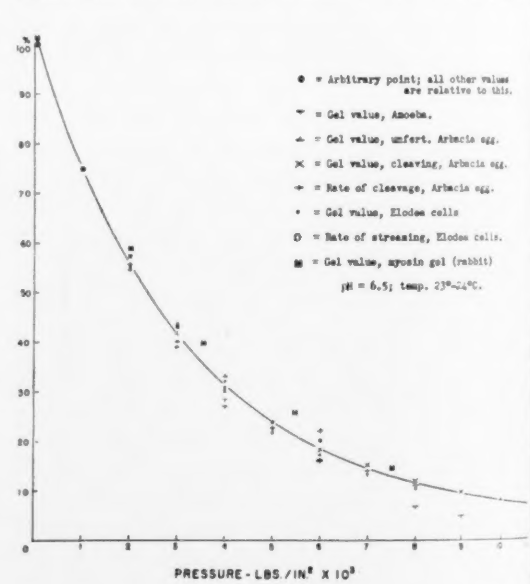


Fig. 9. Quantitative data which substantiate the conclusion that contractility is a function of the protoplasmic gelations. In each case the gel strength at a given pressure is expressed as a percentage of the atmospheric value as determined by the centrifugal methods. Also, rates of activity at each pressure are expressed on a percentage basis.

tude of heavily pigmented cells which enable the fish to change the shade and intensity of its color. By changing its own color to match the patterns of its different backgrounds, the fish can shift from scene to scene in its natural habitat without undue exposure to predaceous enemies.

Figure 10 shows a group of the black pigment cells, called melanophores, in the skin of the common killiefish (*Fundulus heteroclitus*). Here one can see the mechanism of the color change. Each cell possesses a fixed number of branches which radiate from the nucleated cell body, reaching widely out into the colorless area between the cells. If the skin is to assume a very light hue, all the pigment is withdrawn from the branches into the central body of each cell, forming a number of tiny spots too small for the eye to discern except when aided by the microscope. When the fish takes on a very dark hue, the densely pigmented protoplasm of each melanophore expands, refilling the radiating branches, and the whole skin area appears quite uniformly dark when viewed without a microscope.

Experiments performed on intact fish have proved that the metabolic reactions which initiate expansion and contraction in the pigment cells are under the dual control of the nervous and endocrine systems. More fundamentally, however, this expansion and contraction involves an alternate solling and gelling of the protoplasm, as is indicated by pressure experiments on the isolated scales of *Fundulus*. If such an isolated scale is immersed in a weak solution of adrenalin and placed in the microscope pressure chamber, all the cells are seen to be fully contracted, each into a very small, densely packed pigmented mass. As the pressure is gradually built up, one observes a graded ex-

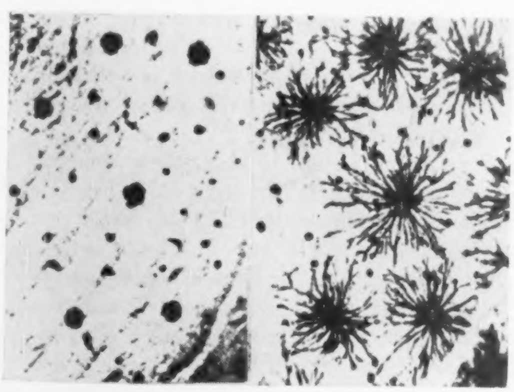


Fig. 10. Numerous black pigment cells (melanophores) in the skin of a fish, which enable the animal to change color according to its surroundings. The fish assumes a lighter hue when the pigment cells are contracted (*left*), and darkening occurs when the melanophores expand (*right*).

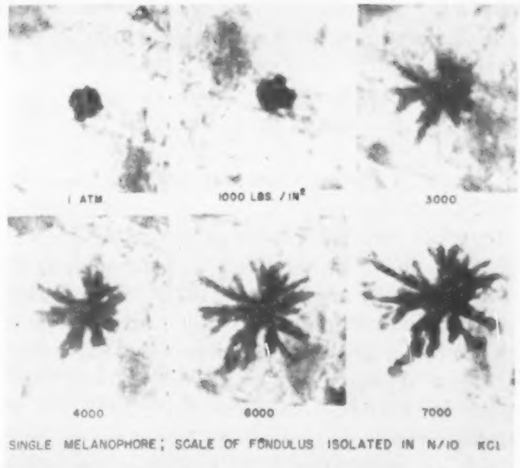


Fig. 11. Graded expansion of the pigment cells which occurs as pressure is gradually increased. This effect, apparently, results directly from the action of pressure upon the sol-gel equilibrium of the protoplasm, which loses contractility in proportion to the reduction of gel strength.

pansion of each pigment cell (Fig. 11). At about 2,500 lbs./in². all the melanophores are about half expanded; a full expansion is not reached until the pressure climbs to 7,000 lbs./in². Moreover, the degree of expansion appears to represent a true equilibrium that can be shifted back and forth according to the pressure conditions. When the pressure is raised and lowered alternately, the same degree of expansion is observed for each specific pressure regardless of whether the pressure is falling from a higher or climbing from a lower level.

Quantitative measurements of the contractility of the melanophore in relation to the strength of its gel structure are not possible: first, because the shape of the cells precludes a free displacement of the pigment granules in the centrifugal field; and, second, because the strength of the melanophore gel system is very great—so great, indeed, that ultracentrifugal forces are necessary to displace the pigment and an ultracentrifuge pressure chamber has not yet been constructed. However, qualitative measurements of the contractility at various pressures give an excellent fit when compared to the pressure-gelation curve for other cells. For example, the half-expanded form is observed at 2,500 lbs./in²., where the gel strength (Fig. 9) amounts to 50 percent of its atmospheric value, and at 4,000 lbs./in²., where the gel strength is reduced 60–70 percent, the melanophores are expanded to about two thirds their full capacity. Consequently, it seems safe to conclude that the movements of unicellular melanophores in fish are

fundamentally similar to amoeboid movement and to the cleavage movements of dividing cells. In each case contractility results from a gelation of the protoplasm, and, when such gelations are inhibited, contractility is reduced proportionately.

ALTHOUGH just one of many protoplasmic reactions has been considered here, it is interesting to contemplate how all the life processes of deep-sea organisms must be adjusted and attuned to the tremendous pressures of their native environment. Deep-sea animals encounter not only high pressures but also low temperatures—so low, indeed, that they approach the limit tolerable to living things. It must be concluded, therefore, that the metabolic reactions of deep-sea forms are modified in such a way that the living processes can still go on despite the unusual extremes of temperature and pressure. Generally speaking, temperature and pressure have opposite effects upon physical and chemical processes, and hence low temperature and high pressure produce additive derangements of the metabolic reactions. If a surface animal is suddenly exposed to truly deep-sea conditions, it experiences a double insult to its life process, and death becomes inevitable. In the course of evolution, on the other hand, natural selection has had ample time to effect a graded adaptation of certain species toward a fitness to cope with the deep-sea environment. But the conditions of temperature and pressure provide an effective barrier that tends to prevent a free migration of species up from, and down into, the abysmal depths. It is not surprising to note, therefore, from the samplings of various dredging expeditions, that oceanic species tend to display a stratification such that species recovered from moderate depths are quite different from those brought up from deeper regions.

Finally, summarizing the more specific results of the experiments on the effects of pressure on the sol-gel equilibria of various cells, it has been found that gelation endows the protoplasm with contractile properties. Thus, protoplasmic gelations are instrumental in the execution of a variety of cellular movements, including cleavage, the transportation of the chromosomes during cell division, amoeboid locomotion, and certain other protoplasmic movements that have not been considered in this account.

CRYSTALLIZATION

A midnight pool of endless depth
Is stirred in transient rhythm.
Within its warming torrents form paths
Of cloudy, nebulous lustre.
Tiny shimmering stars join in ceaseless flight
Caught in a crystal forest—
Star-bound in an ocean fantasy.

CARMEN KENNY

SOME PROBLEMS OF PLANT NUTRITION

D. R. HOAGLAND and D. I. ARNON

Professor D. R. Hoagland, of the Division of Plant Nutrition, University of California, and Dr. D. I. Arnon, of the California Agricultural Experiment Station, shared the Annual Thousand Dollar AAAS Prize in 1940.

DESPITE its rather general title, this article will be confined to observations on the nutrition of higher green plants, and especially to a limited group of these plants—those of economic interest. Discussion of the nutrition of such lower forms of plant life as bacteria and fungi is necessarily excluded from this brief review.

The frequently encountered contemporary lack of appreciation of the fundamentals of plant nutrition has an interesting historical corollary. Despite the agelong association of mankind with crops and forests, the crucial concepts of plant nutrition have been evolved very recently—a little more than a century ago. It was only in the early part of the nineteenth century that evidence was marshaled to demonstrate that plants are made of chemical elements from three sources—air, water, and soil—and, what was particularly striking, that the bulk of plant substance, usually about 90 percent of its dry weight, was made up of the three elements carbon, oxygen, and hydrogen, derived primarily from air and water, rather than from soil. The novel and revolutionary aspect of this concept had to do with the assimilation by the plant of a key element, carbon. It was shown that this was accomplished not by the traditionally known organ of nutrition—the roots—but by the leaves and other green aerial portions of the plant, under the influence of light, during a process now known as photosynthesis.

The failure of some of the most gifted observers and keenest intellects from Aristotle down to comprehend the nature of photosynthesis may serve as an excellent illustration of the importance of the experimental approach, and of the dependence on scientific developments in other fields, to the understanding of nature. No amount of the most careful direct observation of a green leaf exposed to bright sunlight has yielded a clue to its activity as the seat of carbon assimilation. It was only after the discovery of oxygen by Priestley and the elucidation of the nature of combustion and of the chemical composition of water and carbonic acid by Lavoisier, in the latter half of the eighteenth century, that the way was cleared for the understanding of photosynthesis. For this we are indebted to such men as Ingenhousz, Senebier, and De-Saussure.

In the process of photosynthesis the green plant "captures" a portion of the solar energy and, by synthesizing an almost infinite variety of organic carbon compounds, transforms the captured solar radiation into forms of energy usable by man. The green plant is the chief agent in keeping our energy supply from running down in the cycle of life. From the photosynthetic activities of the plant, as they go on now or as they have occurred in past geologic ages, we derive our supply of energy in coal, oil, or currently harvested plant products. By its marvelous synthetic chemical processes the plant yields the complex compounds upon which we depend for our sources of food or other raw materials: for the proteins or their "building blocks," the amino acids, which the animal cannot synthesize (in making these the plant also uses a simple source of nitrogen); for the sugars and other carbohydrates; for the fatty substances; for vitamins; and for other organic compounds essential to the nutrition of the animal. The green plant may indeed be regarded as the supreme chemist presiding over an ever-continuing array of organic syntheses of almost infinite variety and complexity.

The unique role of the green plant has been unchallenged in the past. In recent years, however, the developments in organic chemistry and nuclear physics have occasionally given rise to speculations as to whether this unique position of the green plant will remain without challenge in the future. Nuclear fission is suggested as the alternative source for industrial energy, and artificial chemical synthesis of food as a substitute for natural plant foods. Quite apart from the fact that neither of these contingencies has yet materialized, it is well to keep certain other considerations in mind.

The source of solar energy used by the plant in photosynthesis is atomic transformation in the sun, and in this sense the green plant is already serving as the agent that transforms nuclear energy into forms useful and usable by man. The direct utilization of nuclear fission as a source of industrial energy is still beset by many technical, sociological, and political problems. Assuming that they will all

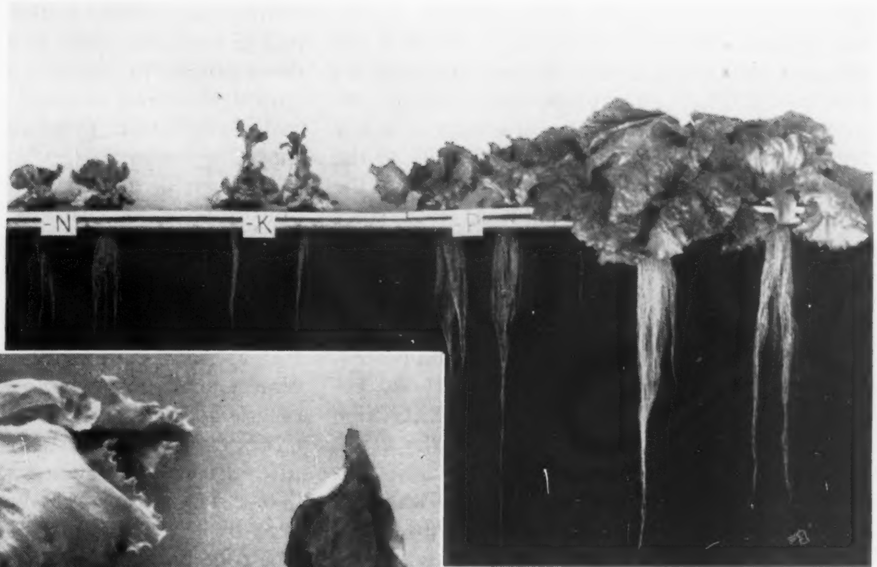
be solved in a manner compatible with the promotion of human welfare, the eventual integration of atomic energy as a source of industrial power into the fabric of modern society appears less revolutionary in consequence than the substitution of synthetic for natural foods. It is true that in recent years several dietary essentials, notably vitamins, have been synthesized by the chemist and are indeed dispensed, often at a cost which compares favorably with that of the natural product. The substances from which they are synthesized, however, are derived from products originally made by the plant in its reduction of carbon dioxide from the air. More important, the vitamins are but a few of the essential food accessories in nutrition, and they are required in rather small amounts. When one contemplates the magnitude of the world's requirements for carbohydrates, fats, and proteins, and the almost infinite variety of natural foods on which man and other animals have learned in their evolutionary development to depend and which they cherish, the size of the undertaking becomes discernible. The chemist would be hard put to it if he had to synthesize the food for the world's population from water, carbon dioxide, and a few inorganic salts, as the plant does.

There is another aspect of this discussion that deserves mention. In contemplating the possibilities of synthetic food production, insufficient consideration is often given to the psychological and aesthetic consequences of such further mechanization of life. It would indeed be a bleak world in which our fields and forests would have to give way to huge industrial establishments, however attractively laid out, to produce the food and timber for human needs. The sustained effort being directed to the study of the mechanism of photosynthesis has not, to our knowledge, among its objectives the displacement of the green plant as the chief converter of solar energy into forms stored in organic carbon compounds.

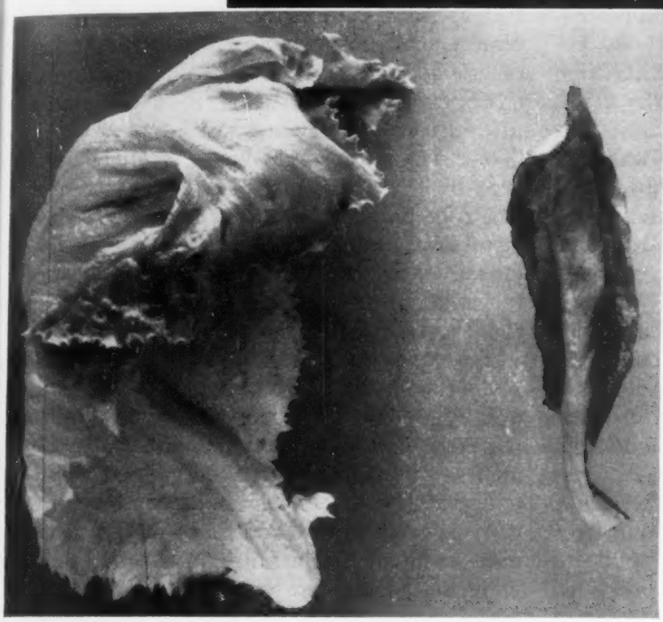
Although life and civilization depend in ultimate analysis upon the photosynthetic activity of plants, popular discussions almost always stress the requirement of plants for certain mineral elements derived from the soil. This is so because often we are able to modify the soil medium in which the plant grows, by fertilization or soil management, so that the yield of the crop will be augmented through an increased supply of mineral elements for the plant; therefore, it has become common to refer to these mineral elements as "plant foods." In reality, the food of plants is the same as that of the animal—carbohydrates, proteins, fats, etc. The difference between plants and animals in this regard is simply that the plant synthesizes its own food as well as that used by animals. The inorganic salts absolutely essential for plant growth serve, in part, as constituents of the organic substances synthesized, and in part as catalyzers of the chemical reactions involved, or else as essential components of the physical chemical system within the plant which must be maintained to provide the internal environment essential for the continued operation of the processes of growth and synthesis.

From the point of view of biological science, the most fascinating task in plant nutrition for the research worker in this field is to learn more of the chemical means used by the plant to carry out the enormous variety of chemical reactions accompanying the growth of the plant—in other words, the study of plant biochemistry. One of the authors has written elsewhere a general discussion of the needs and opportunities for research in plant biochemistry, and merely repeats his observations upon one aspect of this question, namely, that although the basic process of photosynthesis is attracting the attention of a few distinguished chemists or physicists, and although some notable contributions to knowledge of other plant processes have already been made by biochemists, nevertheless, on the whole, the extent of research and progress in the fundamental biochemistry of higher plants has not been impressive in comparison with the brilliant achievements of those biochemists who have addressed their efforts mainly to research on animal biochemistry, or sometimes to that of lower plant forms. Although it seems to be appropriate to emphasize this point of view to give a balanced estimate of the problems of plant nutrition, it is true that at present, to a large extent, plant nutrition (especially of economic plants) is studied in terms of the inorganic requirements of plants, the aspect that has been of most immediate service to those who deal with the problems of agriculture.

The general significance of this research warrants a comment. At the time of the founding of this country, most of the working population was engaged in agriculture; now, according to some estimates, perhaps less than 20 percent of the population is employed in the direct production of food. The immense economic significance of this development and its relation to the growth of modern technology will be readily apparent without extended analysis, although the differentiation between cause and effect would present the difficulties common to all such complex economic questions. This revolutionary change in proportion of the



Showing effect of deficiencies in nutrient solution on growth of the lettuce plant: *N*, lack of nitrogen; *K*, lack of potassium; *P*, lack of phosphorus; *right*, full nutrient solution.



Leaf from lettuce plant grown in full nutrient solution (*left*); from plant supplied all essential inorganic elements except boron (*right*).

total labor force devoted to agricultural production has of course involved many sciences, but understanding of plant nutrition in a broad sense, which includes study of the soil, has a prominent position in this development. As a specific illustration, we think of the use of artificial fertilizers in relation to plant nutrition and soil chemistry, with all that this implies in terms of increased yield of crops.

Another general comment on the study of plant nutrition has been frequently stressed in other discussions by Hoagland and Arnon. Many, even scientific workers in the field, however, do not always recognize this obvious point. The nutrition of the green plant (referring now to plants normally grown in soil) involves perhaps the most complex system studied in any branch of biological science. It is really an attempt to appraise the interactions of three complex systems: the plant organism with all the complexities characteristic of any living organism, together with some attributes peculiar to photosynthetic organisms; the soil system, involving questions related to geology, mineralogy, the properties of colloids, and the growth and effects on the soil of micro- or macro-organisms; and, finally, and frequently of decisive importance for plant growth, the aerial conditions —light intensity and quality, temperature, carbon-dioxide concentration in the atmosphere, wind movements, and humidity. Presiding over the whole system of soil-plant-atmosphere is water in its various forms.

To disentangle the interactions of the several systems is a challenge to any research worker. Indeed, the difficulties are so great that often the most gifted workers are likely to avoid this field of study in favor of fields where the exactness of knowledge and the possibilities of control are greater. But at this period in the world's history no special pleading is needed to make a case for any science that may aid in the production of more and better nutrition for all the human inhabitants of the earth.

Historical background of plant nutrition. As already pointed out, the scientific basis of plant nutrition, as we know it at present, was long shrouded in obscurity. That is not to say that even the ancient civilizations did not take advantage of a few ideas of plant nutrition learned empirically in the exercise of the agricultural art; for example, the frequently observed beneficial influence on the soil's productivity of the growth of leguminous plants, the often favorable influence of animal manures or of plant ashes on crop growth, and the like.

Before we learned that the plant derives its carbonaceous compounds from photosynthetic processes, there were for many centuries wholly erroneous or semimystical views of plant nutrition held by those who thought about the matter at all. One ancient idea, which indeed persisted until perhaps a little more than a century ago, was that plants absorbed their foods partly preformed from the juices of the soil. There was also the era in which natural philosophers interested in plants sought as a primary objective for some so-called "principle of vegetation."

Van Helmont (1577–1644) performed a famous experiment—and it was for its time a noteworthy one—which led him to the false conclusion that the principle of vegetation was water; in other words, that the substance of plants was made from water alone. This conclusion, however, was doubted by another investigator, Woodward, as early as 1699, when he grew certain plants in river or conduit water, in comparison with water to which garden mold had been added. His work led him to believe that plants require certain "terrestial matter" for their growth, and that water alone was not the "principle of vegetation."

This experiment was an early precursor, in a very crude form, of modern investigations which have had as their objective the determination of the inorganic requirements of plants that are absolutely indispensable to their growth, as well as an explanation of the process of photosynthesis. About 1860 the plant physiologists Sachs and Knop elaborated the so-called water-culture method by which plants are grown with their roots, not in soil, but in a solution of nutrient salts. This became one of the chief methods by which the inorganic needs of the plant were learned. It is still among the most important tools of the student of scientific plant nutrition.

It will be recalled that through an injudicious flare of publicity, many people first became aware some years ago that plants of economic interest could be grown without soil, and were led to the belief that perhaps an artificial method of plant culture, long known to the plant physiologist as a tool of research, could be employed, with further development, to supplant commercially the ordinary agricultural methods of growing plants, and that it might be of great aid in increasing the food supply of the world.

Left out of adequate consideration were questions of economic cost and difficulties of technique in growing plants commercially in this way. In fact, many of those who were most enthusiastic about the possibilities of this development had little or no basic knowledge of plant nutrition. It is true that artificial culture methods have been used to some extent in growing certain high-priced crops, especially in greenhouses. Artificial culture methods (gravel culture, usually, rather than water culture) have also been tried in a few places out of doors, such as on coral islands, where no good soil is at hand, especially as a result of war or occupation conditions, in which the competitive economic factors are not operative. But a widespread popular misconception of this subject went occasionally to fantastic extremes and betrayed a lack of even elementary knowledge of plant nutrition.

After the general concept of photosynthesis was established in the last century, investigators turned to a systematic search for the inorganic elements required for plant growth, principally by the method of controlled artificial culture, as already indicated. In the earlier stages of these investigations, however, the control of the nutrient solutions was limited. The nutrient salts used contained impurities, and there were also other sources of contamination. Thus the experiments seemed to show that, as a general mineral requirement for the growth of the higher plants studied, only seven elements provided by the nutrient medium were needed by plants in addition to carbon, hydrogen, and oxygen derived from air and water. As we now know, impurities in nutrient salts or from other sources supplied the additional needed elements. The seven inorganic elements considered indispensable were nitrogen, phosphorus, sulphur, calcium, magnesium, potassium, and iron—all present in the nutrient medium, of course, in the form of their salts. It seemed, therefore, for a long time that in total the plant required only ten chemical elements as indispensable: the seven inorganic elements provided by the culture medium as salts, and carbon from carbon dioxide, with hydrogen and oxygen from water. Our present definite knowledge that plants also require minute amounts of certain other chemical elements is a comparatively recent development of great scientific interest and practical use.

It may be interesting to introduce this aspect of plant nutrition in terms of a few personal experiences in which the authors have participated, although the first indication that the plant required minute amounts of chemical elements not included in the so-called classical list was obtained long before they began their work in this field. One of the writers had been engaged about a quarter of a century ago in studying plants growing in culture solutions in connection with a project that had as its purpose an attempt to evaluate the characteristics of the supposed nutrient solution of soils, the soil solution. Utilizing an artificial solution intended to have a resemblance to the soil solutions of a group of soils under study, marked success was had in growing barley and a few other species of plants in the artificial solution. It was then decided to use this solution for growing bean plants. These plants failed to grow after a short time, however, and the cause was not evident from any information generally available at that time.

About this time, the writer in charge of these experiments made a trip to Europe, and at the famous Rothamsted Experimental Station, near London, he was shown some experiments in which broad-bean plants were growing in different nutrient solutions, of the same composition except that certain solutions contained a minute amount of boron in addition to the other nutrient elements then considered to constitute a complete nutrient solution. The failure of the plants without boron and their good growth with boron was strikingly apparent. The technician in charge in California was instructed by letter to try to grow bean plants in the California nutrient solution in which the bean plants had previously failed, but with addition of a minute amount of boron. The bean plants then grew well. As we are now aware, boron is apparently required by all species of higher plants. The bean plants merely had a larger requirement than barley and some other species of plants.

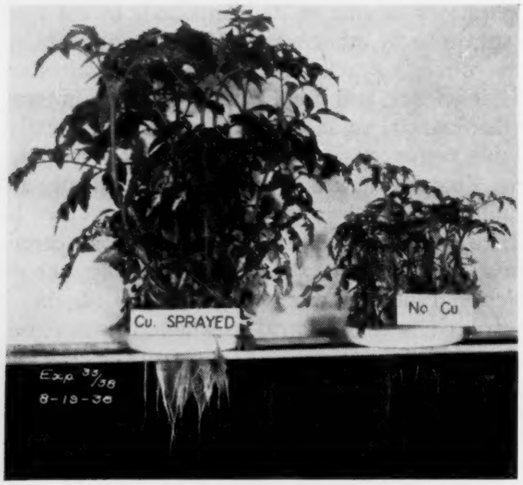
Passing over a long period of time, the laboratory with which the authors are associated had occasion to cooperate in the early thirties with W. H. Chandler, professor of pomology, on a study of a serious disease of deciduous fruit trees, often called "little leaf." Partly owing to accidental circumstances of an experiment, it was discovered that this heretofore highly obscure disease was in fact caused by a zinc deficiency and later, along with other investigators, it was discovered that a related disease, of great economic importance, the "mottle-leaf" of citrus, was also a zinc-deficiency disease. Frequently these diseases can be cured or prevented by applications of zinc or its compounds in the form of sprays or by other means. These treatments are now common practice in the orchards that show this deficiency, and if one wished to speak in terms of the financial value of the commercial application of this discovery, it might represent a very large sum. Zinc deficiencies were also independently and simultaneously observed by workers in other locations and on other species of trees. In certain areas, field crops may also be affected by zinc deficiency. Indeed, there are agricultural regions in several parts of the world in which, in some soils, various crops respond to zinc treatment, although ordinarily the zinc deficiency is of dominant interest in the culture of fruit trees.

Before the essentiality of boron and zinc was established, several investigators had secured evidence that manganese was an essential element for plants; there is also considerable evidence that copper must be added to the list of essential elements.

A much more recent development was the discovery by Arnon and his colleagues, P. R. Stout and others, that certain higher plants, if not all, when grown in nutrient solutions, also need the element molybdenum for their growth. This view was confirmed by Australian workers and, very recently, also by English and Dutch investigators. The Australian workers have also found that in some of their soils certain species of crops respond remarkably to very small applications of molybdenum to the soil.

The practical agricultural significance of investigations with the inorganic elements needed by crops in minute amounts, sometimes called the micronutrient elements, is of a great import in terms of increased production of crops in deficient soils, even though deficiencies are present only under special soil conditions and not to be expected in all, or even in most, soils. A full discussion would far exceed the limits of this article, and recently Walter Stiles has published an excellent monograph treatment of the whole subject (*Trace Elements in Plants and Animals*. New York: Macmillan, 1946).

There is, however, another general aspect of the development of research on the micronutrient elements, now of so much practical agricultural interest, which deserves recognition in this journal. That is that, correlated to the subsequent practical applications, there existed a foundation of knowledge in this field in the research of several investigators who seemed at the time to be doing work of a wholly academic nature. Among these investigators were A. L. Sommer and the late C. B.



Tomato plant (*right*) grown in purified nutrient solution, without copper; plant grown in same solution, but sprayed with very dilute solution containing copper (*left*).

Lipman, at one time working in the authors' division of the College of Agriculture. Intensive and laborious research by these investigators determined the need by plants for certain of the micronutrient elements—in work which demanded the utmost refinement of plant-culture technique. Special purification of nutrient salts, particular care in the preparation of water by distillation, and avoidance of other contaminations were all essential to the purpose of the investigation. It was necessary to show that the plant under study could not complete its life cycle without the element in question, that the element was directly required for the nutrition of the plant, and that no other chemical element could be substituted for it. Truly this was an exacting task.

Some of the discoveries that have led to the agricultural applications of the micronutrient elements have had an element of the fortuitous, but progress even in the practical field would not have been so rapid or so assured without the preceding or accompanying basic, or so-called academic, research. Of course this is only the usual course of science and technology, but it seems appropriate to call attention to this further illustration in the biological field of science.

Interrelations between plant and animal nutrition. There is a phase of research on the micronutrient elements, as well as on elements absorbed by the plant in larger amounts, which has an application broader than that of plant nutrition as such. We are referring to the use of the plant as food for humans and animals. The seed and fruit of a plant are generally not subject to large alteration in their composition by changes in the nutrient environment, but the vegetative portions of the plant may be considerably modified, depending on the nutrient medium and its ability to supply mineral elements to the plant. Hence, there may sometimes arise in deficient soils a deficiency of certain mineral elements for the food of the animal, especially for grazing animals, such as deficiency of calcium, phosphate, iron, copper, manganese, iodine, and in a few cases even cobalt, although it is not proved that the latter element is essential to the welfare of the plant itself.

There are special regions in which these deficiencies become of economic importance in the feeding of some animals. Because of the variety of foods from many sources usually consumed, their significance for human nutrition is less apparent; nevertheless, it has been a subject for consideration. Indeed, the whole question has aroused sufficient interest to lead the Federal Department of Agriculture to establish a special laboratory at Cornell University for the investigation of the interrelations of soil, plant nutrition, and animal nutrition. And recently there has appeared an announcement that a large grant has been made to Johns Hopkins University for the study of "trace" elements, to use another term for what we have called the micronutrient elements.

These interrelations are not limited directly to mineral elements derived from the soil, and there has been some discussion of the possibility that the environmental conditions, including the mineral nutrition of the crop plant, may modify its content of organic substances important in the food of humans or animals. Some of the vitamins, especially, have received attention in this regard. To amplify the general point already made, at least as far as food plants are concerned, there is no consistent evidence that the vitamin content of the plant is mainly influenced by its mineral nutrition. On the contrary, much of the existing data support the view that the vitamin content of a plant (vitamin C, particularly, has been investigated) depends primarily on its heredity—that is, the inherent species and variety characteristics—and on the climatic conditions under which the plant is grown rather than on its mineral nutrition.

This is not at all to say that our present knowledge of this aspect of plant nutrition is adequate, but to suggest caution if the claim is made, for example, that the most nutritious food can be produced only when organic fertilizers are used instead of the artificial fertilizers so commonly employed. This is a field of discussion in which it is

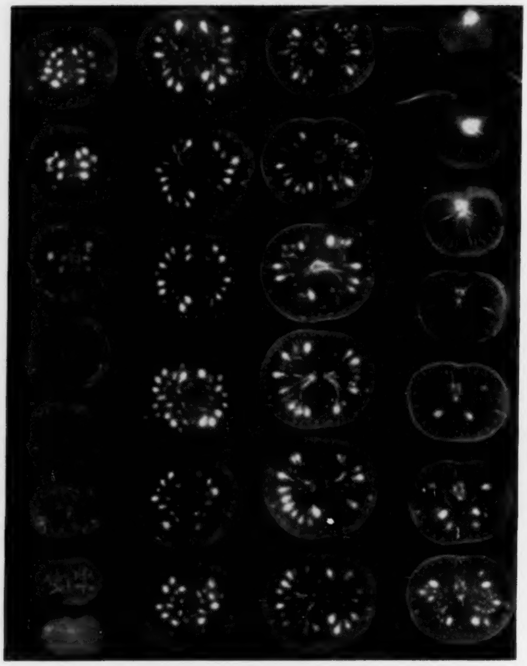
Tomato plant (*left*) grown in full nutrient solution with stream of air continuously bubbled through solution; (*right*) plant grown in same solution without air.

easy to make broad generalizations without adequate scientific evidence. To obtain really convincing evidence is most difficult. The questions involved are the most complex of all, since they relate to the system soil-plant-atmosphere and animal. As stated earlier in this article, the green plant is predominantly a synthetic organism, making its own food and that used by animals, or, in more technical terms, such plants are autotrophic.

Nature of the process by which plants absorb inorganic nutrients. One of the scientific questions of outstanding interest for understanding plant nutrition concerns the nature of the processes by which plants take up from the soil or other nutrient media the inorganic elements they require for growth, or other elements present in the medium and absorbed by the plant but not indispensable to it. Parenthetically, it may be said that the plant does not absorb only those elements indispensable to its own growth, but may take up other chemical elements present in the medium, even perhaps when they are toxic to the plant itself.

The older concept often was that plants absorbed inorganic elements along with water, in something akin to a wicklike action. More recent investigations, in which the writers and others have been engaged, show that this simple concept does not express a true idea of the plant's ability to absorb mineral elements from soil or from nutrient solutions. A variety of experiments have been carried out, including those done with excised roots of plants, with storage tissue, and with certain algae which develop relatively huge cells that permit direct observation on the contents of the vacuole. (The latter is mainly a solution of salts in the species of algae investigated.) In the algae, the vacuole is clearly separated from the nutrient solution (in fresh-water algae, this is in nature pond water) in which these plants grow by a layer of living protoplasm and a cell wall. Because of the large size of these plant cells, or vesicles, direct observations are possible on the external and the internal medium of the vacuolar sap. Higher plants are made up of cells so minute that similar direct observations cannot be made, but there is reason to believe that an analogy to the large-celled plants has validity.

The amount of research given to these phenomena of salt absorption has been great, but only a



Radioautograph of tomatoes from plant given minute amount of radioactive zinc, showing the location in the tissues of the radioactive element.

few words can be said here in summary of one of the general conclusions of many experiments. It does not seem that the normal living plant acts through its root system merely like a passive wick. On the contrary, the roots can absorb mineral elements in an active sense, in that they may move the salts from a nutrient solution (which may be very dilute) into the vacuole or other internal aqueous phases of the living cells, not alone by a process of diffusion proceeding from a region of high concentration to one of lower, but also by processes in which the living cells have the remarkable ability to move the inorganic elements (at any rate, some of them) by what may be called an "uphill" process, *against* concentration gradients. This necessarily means that in so doing the plant must expend metabolic energy, and this energy must be derived from the energy of cellular respiration. There is much to indicate that this view is correct.

The practical agricultural interest of this research inheres in giving another scientific foundation for the practical observation that most agricultural plants thrive only when their nutrient medium is well aerated. For this, among other reasons, the porosity and physical status of the soil are of great importance as essential determinants of the properties of a medium in which a plant will thrive. To be sure, there are plants of a few species, like the rice plant, that may grow well in submerged soils, not well aerated. They may in part derive energy from anaerobic fermentative processes or they may not be internally anaerobic in their root systems, because there may be a system of internal aeration in the plant, with oxygen transported from the shoot—where of course it is produced in the process of photosynthesis—to the root.

There are other consequences of the view that plants have the power of active absorption of solutes, requiring a source of metabolic energy. It contributes to our scientific analysis of the interdependence of the top of the plant, where the organic metabolites are manufactured, and the activities of its roots. For the agriculturist it helps to clarify further why it is important, in understanding the so-called availability of mineral nutrient elements in the soil (or added as fertilizers), to consider that this is not merely a question of soil chemistry, but also of those conditions, particularly climatic conditions, governing the synthesis and translocation of organic compounds such as sugars, amino acids, and growth substances from the synthetic parts of the plant to the roots. There they serve for those metabolic activities associated with the processes of growth and salt absorption by the plant. The growth processes in the root also lead to an extension of contacts of root and soil which is of great importance to the absorption of mineral elements by the plant.

The use of isotopes in research on plant nutrition. The reader, even if he has only a slight knowledge of plant nutrition, may be inclined to ask: "But is there not something newer and fresher in the scientific field of plant nutrition?" Chronologically, this question can be answered in the affirmative, even though the general aspect of plant nutrition in mind can scarcely be unknown at this time to anyone interested in scientific development at all, and some pioneer experiments were made long ago with naturally radioactive elements.

We refer, of course, to the recently enlarged possibility of studying certain of the plant's chemical or physical processes with the aid of artificial radioactive isotopes or, occasionally, with stable isotopes. A new tool is provided to study plant nutrition and plant physiology, as well as animal nutrition and physiology. This tool, as far as the artificial radioactive isotopes are concerned, was stimulated by the cyclotron production in quantity of artificial radioactive isotopes of many types to permit their use in biological research.

and more recently, and to a far greater extent of general applicability, as a by-product of developments associated with the making of the atomic bomb.

Radioactive elements have the same chemical properties that their nonradioactive counterparts have, but the radioactive isotope can act with the highest degree of sensitivity as a "tag," or tracer, in the plant or animal organism. The purpose of detection of the radioactive tracer element is served by the fact that the radioactive atoms emit radiation with energies ranging from one-tenth to several million electron volts. Instruments of the type of the Geiger-Müller counter can be used to detect this radiation. The ultimate sensitivity of detection of the total amount of a radioactively tagged element is directly proportional to the ratio of radioactive to nonradioactive isotopes in a given sample. Some of the radioactive tracer elements used may represent the same elements utilized normally by the plant in nonradioactive form, such as phosphorus and potassium. Thus the reactions of these elements as they would occur normally in the plant can be traced.

Not only may the movement of an element be traced by such means, without in some experiments destroying the plant, but also it is a property of radioactive substances, through their emission of positrons, beta particles, and gamma rays to activate suitable photographic plates. In this way, intact leaves or sections of plant tissues containing radioactive substances can make their own pictures, called radiographs, which help to explain the way in which the element moves in the plant or is accumulated by it. This method has not yet been used for precise quantitative work, but it is most enlightening for certain types of experiments. P. R. Stout prepared what were apparently some of the earliest radiographs of plants in this manner.

As illustrations of the kind of information made available by the use of radioactive isotopes, there may be cited experiments to determine the rate at which inorganic nutrients accumulate in tissues of varying metabolic activity, and the tissues through which they are transported upward or downward. An application to a problem of direct agricultural interest is the study of the fixation by the soil of certain mineral nutrient elements, such, for example, as phosphorus, and the location in the soil of the parts of the root system utilized in removing from the soil a fertilizer element that has been applied to the soil. The usefulness of the technique for this purpose could not be fully apprehended without a discussion of reactions of soil colloids and fertilizers and the fixation of chemical elements by the soil outside the scope of this article, but the practical significance is obvious.

There are some elements of great importance in research in plant nutrition or in biological research as a whole which have not been prepared in the form of radioactive isotopes of sufficiently long half-life to make feasible their tracing in biochemical or physiological processes in the plant for a suitably long period of time, before the radioactivity has decayed. Nitrogen is a particularly important example. There is the possibility, however, of utilizing in some such cases, compounds tagged, not by their radioactivity, but by containing a higher-than-normal percentage of a rare, stable, heavy isotope of the element in question. To determine that a compound has been enriched with the stable isotope it is necessary to employ a mass spectrometer: work is being inaugurated on certain problems of plant biochemistry in the laboratory of the authors at the present time. This method has already had an important role in researches of various other laboratories. Notable investigations are being made in animal metabolism by the group formerly headed by the late Dr. Schoenheimer at Columbia University.

In considering these remarks, the general reader should bear in mind that the use of isotopes, radioactive or stable, in biological investigations does not mean that a sort of magic has been introduced that will automatically result in the solution of heretofore insoluble problems. The isotopes simply form a new tool of unique properties, which is most effectively employed in the light of fundamental knowledge already developed by other means and with the concurrent aid of other techniques applicable to the problem in hand. The potentialities for use of the new tool are of extreme value for certain research, sometimes enabling information to be gained that without the isotope technique would remain inaccessible. But their introduction should perhaps be appraised with a more balanced judgment than occasionally may be formed from reading popular accounts purporting to make clear the revolutionary effects on biology of the discovery of artificial radioactivity.

EXPERIMENTALLY INDUCED ABNORMAL BEHAVIOR

NORMAN R. F. MAIER

Professor Maier, of the Department of Psychology, University of Michigan, was the recipient of the Annual Thousand Dollar AAAS Prize in 1938.

MOST of what we know about abnormal behavior has been learned from the study of mental patients and the way they respond to various forms of therapy. There are, however, a number of problems concerning the nature of the abnormal that can only be solved by experimental procedures with animals. One of these is to determine whether neurosis is a disease peculiar to man. This raises the question of whether man is so subject to the disease because of his superior mentality or whether the disease is primarily the product of man's way of living. It has been said that it takes imagination and intelligence of a high order to experience conflicts and that personality disorders require a complex personality structure. Obviously, the ability to produce true neurosis in animals will make it possible to answer this question.

The second problem concerns the relation of the symptom to the cause of the symptom. Has nature supplied man with protective mechanisms—processes whereby the organism develops certain unusual responses that serve to prevent a worse condition? Thus, if a patient develops hysterical blindness, is this symptom a means for protecting the patient from seeing something in his environment that causes his anxiety and conflict? Further, does a psychosis represent a patient's escape from reality and hence serve as a solution when reality is too stressful? If one deals only with studies of case histories, one can find support for this thesis, because the many events in a case history permit one to find a logical connection between the symptom and some problem in the patient's life. Further, when a child has enuresis, should one seek in this behavior some reason, or some way in which he thereby solves a problem? Could bed-wetting give him attention he desires? Could it be a way of striking back at strict parents? Only when we control the life histories of individuals and purposely produce symptoms can the relationship between a symptom and its cause be studied.

Animal studies permit one to secure the necessary case-history data for such investigation, but in order to use lower animals one must be able to produce behavior disorders comparable to those found in man. Thus, one of the first steps in animal studies of abnormal behavior was acquiring the ability to produce abnormal symptoms under laboratory conditions.

When our work was begun, success already had been achieved in this field. By the use of the conditioning method, Pavlov produced what he called an "experimental neurosis" in the dog. Liddell's laboratory at Cornell followed this line of attack, and Liddell used sheep and pigs as well as dogs in his research. At Johns Hopkins University, Gantt established a laboratory to continue the type of research initiated by Pavlov. These studies clearly showed that stressful and conflictual situations caused basic behavior disruptions. The disturbance was apparent from the facts that training on discrimination problems was lost, the animal became emotionally unstable (struggled and bit at restraining harnesses), and, in general, the docile animal became most uncooperative. Many symptoms akin to those observed in human patients (such as disturbances in heart rate, and peculiar fears) were seen, but their appearance was difficult to separate from reactions of normal individuals. At the time that our work with rats was begun at the University of Michigan, there was some doubt as to whether the changes produced in the animal were a "true" neurosis. The symptoms were regarded as not sufficiently profound to be convincing, and some psychiatrists argued that neurosis in subhuman animals was impossible because animals below man lacked sufficient imagination, they could not have sex conflicts, or they did not undergo permanent personality changes. Even though some of these criticisms seem to depend upon specialized definitions of neurosis, which exclude the disease from lower animals by defining it as a human disease, it seemed at the time that part of the failure to accept neurosis in animals was based upon the facts that the behavior disturbances observed were not profound enough and that the behavior was produced by a specialized aspect of the conditioning method.

IT IS probable that the attention our study received in 1938 was influenced by the fact that the disturbance produced in the rat was extremely violent and left no doubt about its being abnormal.

The abnormal behavior was in the form of a seizure in which the rat ran madly in a circular pattern. This running was so violent that the nails of the feet became torn. It was not a typical fear pattern, because shelter was not sought and the animal often ran into table legs and walls. The running phase frequently was followed by a convulsion (Fig. 1), which was similar to that produced by drugs; since then we have actually found that the convulsions produced by metrazol have a good deal in common with those produced in the training situation. After the active part of the seizure had passed (one or more minutes), the rat became very passive and its righting reflexes were either absent or greatly depressed. During this period the animal could be molded into almost any position, where it would remain for several minutes—sometimes as long as twenty minutes (Fig. 2).

The situation for producing this behavior was built around our interpretation of the conditioning studies. It seemed that behavior disturbances in the earlier studies arose when an animal was trained to give a certain response to a signal and to withold the response when a different signal was given. When these signals were made more and more alike, the animal had difficulty in determining whether to express or withold the response. By the presentation of a signal that was as much like the withholding signal as the arousal signal, the animal was stimulated to express and withhold the response at the same time. This was a conflict between doing and not doing which we regarded as a basic conflict.

To incorporate this condition in our experiment, we trained rats to discriminate between the two

Fig. 1. This rat is in the last stages of a convulsive seizure. Previous to the convulsion, it leaped from the discrimination apparatus and ran wildly about the floor in a circular pattern. It then fell on its side with all legs kicking. At the time picture was taken the rat was beating the floor with its forelegs and whipping the floor with its tail.

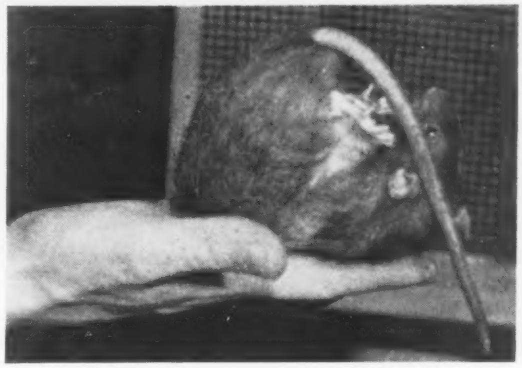


Fig. 2. Rat in the passive state. When placed in an awkward position, as shown in the photograph, it remains so without showing its normal righting responses. The passive state is more profound and of greater length when a convulsion follows the running stage.

cards in the apparatus shown in Figure 3. The rat was trained to jump to the cards, and when it chose the correct card (the one with a white circle) this card yielded to the jump and gave the rat access to food. When the rat jumped to the wrong card (the one with the black circle), this card remained in place and the animal received a bump on the nose and fell into a protective net below. Under these conditions animals soon learn always to choose the reward card and to avoid the punishment card. After the discrimination is well learned the situation is changed so that only one of the cards is presented (Fig. 4). If this card happens to be the punishment card, the animal, as may be expected, refuses to jump. In order to cause the animal to jump to the punishment card, it is necessary to drive him. This is done by using a jet of air and directing it on the rat. When released, the air makes a hissing noise and is irritating to the rat; consequently the resistance to jumping is broken. The condition of driving the rat to make a response to a card it has been trained to avoid was considered a conflict between doing and not doing, and it was the condition under which the violent seizures most frequently were produced.

These experiments raised two interesting questions: Was the seizure produced by the conflict, or by the sound of the air? Is the seizure akin to an epileptic attack and, therefore, an abnormality other than a neurosis? The implication of the second question is that the symptoms observed are too profound to be considered a neurosis.

Experimental studies concerning these and other points have been numerous. Our laboratory alone has contributed more than 25 studies, and a total of perhaps 150 studies have been published on some aspects of the rat's abnormal behavior. The studies include the effects of diet, drugs, heredity,

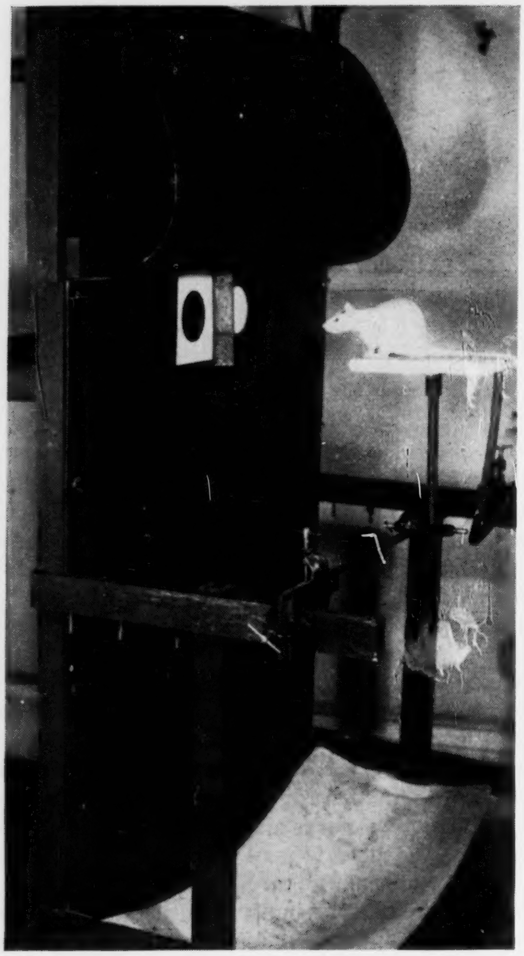


Fig. 3. Photograph of rat on the jumping platform confronted with discrimination cards. The platform behind the large screen is the feeding station which the rat reaches if it strikes the unlocked card. The net near the bottom of the picture catches the rat when it jumps against the punishment card, which is locked in place. Toward the back of the jumping platform, air is led through a hose, and a small nozzle (not visible) directs the air against the back of the animal. Close scrutiny will reveal the ruffled hair caused by the air.

emotionality, and brain injury on seizure susceptibility, as well as studies directed toward determining the nature of the abnormality.

At the present time it seems quite clear that the basic condition for producing the disturbance is conflict. The same auditory conditions, with and without the element of conflict, produce different results. The delay in settling this issue was due to the fact that auditory stimuli, such as the hiss of air, the sound of buzzers and bells, the jingling of keys, supersonic tones, and pure tones of low pitch all produce seizures in some animals. To explain these seizures it was necessary to show that these conditions also produce conflict. Certain sounds are irritants and arouse generalized escape behavior. However, when the animal is confined it is driven to escape, but at the same time its escape is blocked. Thus the animal is trapped in a situation which demands responses and yet inhibits those responses. It has been shown by Dr. Marcuse, of Cornell University, that seizures do not occur when the sound source is fastened to the animal. Under these conditions the escape behavior is permitted even though escape is not accomplished. It also has been shown that the type of confinement and the type of responses made during auditory stimulation influence the appearance of seizures. These facts indicate that auditory stimulation as mere sound stimulation is inadequate for producing seizures. Rather, other conditions must be present, and these other conditions determine whether behavior tensions are built up without permitting a release through some avenue of behavior. It is when these tensions become too great that they break forth as a seizure. The fact that smoke, water spray, and electric shock (applied during conflict) also produce seizures under proper conditions indicate that sound is not unique in its seizure-producing qualities. Since the expression of escape behavior prevents seizures, one is led to conclude that behavior must be blocked while irritants are applied.

The mere fact that conflict and unresolved tensions seem to be essential for producing seizures makes the seizure appear to be a form of neurosis rather than the epileptic attack of a defective organism. Perhaps Dr. Goldstein's classification, "catastrophic reaction," is more adequate than neurosis, since this term implies a form of disorganized behavior which occurs when the environment places demands on the organism that it is incapable of handling. Either "neurosis" or "catastrophic reaction," however, places the emphasis on the abnormality as being one that is situation-induced rather than the response of an injured or defective organism.

Because of the profound nature of the seizure other aspects of the abnormality reported in our rats frequently have been overlooked. The conflict situation in the rat was highly frustrating and produced nervousness which extended outside the situation. Experimental rats become less likely to breed, and they develop a retiring nature. These behavior alterations are akin to personality changes, since they extend outside the test situation. Further, and more important, is the fact that the frustrating situation produced compulsive behavior in many of the rats, particularly those not showing seizures. Compulsive behavior commonly

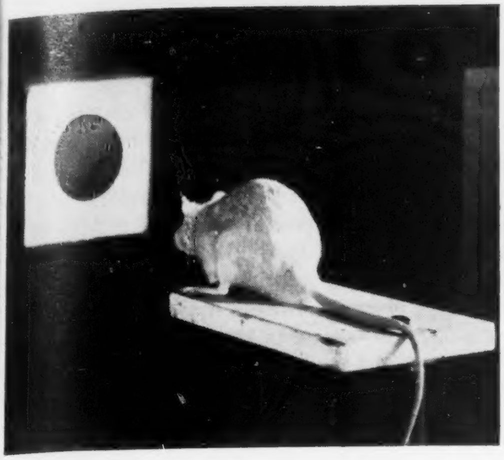


Fig. 4. Close-up of rat in the one-window test. In this test only one card is presented at any one time. The animal is about to be forced to jump to the punishment card. (Nozzle attached to the air hose is shown beside rat's tail.)

appears in neurosis and is one of the most difficult to explain. A classical illustration of compulsive behavior is Lady Macbeth's repeated hand-washing, which is regarded as an effort to cleanse herself from guilt. Because compulsive behavior is a fundamental type of abnormality, it was selected for special study. Thus, from the outset, our studies of abnormal behavior in the rat have dealt with two distinct forms of abnormality.

When a rat is placed in the card-discrimination problem situation in which reward and punishment are applied in a random order, the animal is confronted with an insoluble problem. This fact is soon recognized by the animal, and it expresses its recognition of such a difficult problem by refusing to choose between cards. This refusal is so intense that hunger is not sufficient to cause the animal to take a 50–50 chance on happening to strike the reward card. In order to overcome this resistance, the animal is driven with a blast of air, as described earlier. Occasionally, seizures are produced in this situation, but more commonly the animal jumps at one of the cards, and soon its choices follow a consistent pattern. Usually the rat chooses a card on a position basis, i.e., it chooses the card on the right (or left) side, regardless of which of the cards it is. Once the rat ceases trying out various possibilities and makes its choice on a position basis, this way of choosing becomes the response to the insoluble problem, and the animal never deviates from this procedure once it is established. In practicing the position response, the animal is punished on half its choices and rewarded on the other half, since these are applied in random order. Such a condition should not establish a preferred way of responding, yet under frustration a highly specific response becomes established.

Other animals can be *trained* to show similar position responses. This is accomplished by rewarding animals for choosing the cards on a position basis. Such animals are *motivated* to express position responses, whereas the animals in the insoluble problem situation express their position responses as a consequence of *frustration*.

The question now is, Are responses established under frustration different from those established through motivational training?

It is found that animals that acquire their responses under frustration cannot substitute them for other responses. In other words, they cannot learn new responses even when the situation ceases being insoluble. Not only are they unable to adopt new responses, but they are unable to drop their inadequate position responses. This is true even if they are punished each time they express their old responses. As a matter of fact, punishing them for making their former responses makes them *more* likely to repeat them in the future. Animals with such frustration-induced position responses will choose the punishment cards whenever they are placed on the side of the animals' position preferences. They will even refuse to jump to an open window in which food is clearly displayed when its position does not correspond with their position reaction. This apparently foolish response is illustrated in Figure 5. Before jumping to the punishment card this rat sniffed longingly at the food.

This rigid behavior is in contrast to that of animals that have acquired their position responses under conditions of motivation. These animals readily learn new responses when training condi-

Fig. 5. Rat jumping to the punishment card, which is placed on the side of its right-position fixation. The strong tendency to jump to the right card makes it impossible for the rat to jump to the open window on the left, in which food is exposed. Before jumping to the punishment card, this rat sniffed toward the food and then quickly jumped to the right.

tions are changed, and they are constructively influenced when being punished for errors. If punished too severely, however, they, too, may become frustrated, and they then behave like the above-mentioned animals.

Because frustration makes behavior rigid and unchangeable, we have called the responses acquired under frustration "abnormal fixations" to distinguish them from normal habits. The adjective "abnormal" is used because the strength of the response does not follow the principles of learning in establishing or fixating habits.

Abnormal fixations not only are rigid but they have a compulsive character. This trait can be demonstrated in the following manner. Suppose that after a period of frustration a rat's jump to the card with the white circle is always rewarded, whereas a jump to the card with the black circle is always punished. In this situation a rat with a right-positional fixation will receive punishment whenever the card with the black circle is on the right side, and reward whenever the card with the white circle is on the right side. After a time in this situation, the rat begins to hesitate to jump whenever the punishment card is on the right, or position, side. When forced to jump it strikes the card with its rump (Fig. 5) to avoid a bump on the nose. When the reward card is on the right side, however, the animal jumps readily and hits the card with its nose and forepaws. It is evident that the animal knows which card punishes and which card rewards, and, although it expresses its knowledge of the difference in the cards by the way it jumps, it does not choose the card to jump to on this basis. The right-position fixation apparently prevents the rat from making an adaptive response to the situation, so it is forced to take punishment even though it knows better. This unadaptive behavior is in contrast to that of animals with normal position habits. As soon as they learn which card punishes and which card rewards, they abandon their position responses and, instead, follow the reward card from right to left.

Behavior similar to the abnormal fixations in rats has been demonstrated in college students by Dorothy Marquart, one of our graduate students. After mild frustration in any insoluble problem, the time required by students to learn a simple problem is greatly increased. Since all learning requires the acquisition of a new response, any resistance to change that is produced by frustration results in retarded learning. As might be expected, individual differences were apparent. Some of the students were not frustrated by the mild shock and learned at the normal rate. Those that were frustrated, however, required many more trials than the slowest of the normal learners.

The abnormal fixation is akin to rigid responses found in human beings. Accounts of compulsive behavior, such as is found in kleptomania, phobias, and alcoholism, are common in the literature of abnormality. So-called ritualistic behaviors, in which the person must repeat a senseless routine of activities, are further examples. The fact that some attitudes are rigid and not subject to modification, regardless of how senseless they are from a logical point of view, suggests that they are fixated. Of interest are the facts that rigid attitudes are emotionally loaded and are commonly associated with objects that are threats to a person's security. These observations support the suggestion that rigid attitudes have their basis in frustration. Thus, attitudes on socioeconomic topics, racial questions, and religious questions are least subject to modification, and these attitudes are most rigid during periods of frustration and stress.

THE studies of abnormal behavior in the rat lead to a new theory of frustration. They demonstrate that behavior elicited during a state of frustration has certain unique properties, and that these properties make frustration-induced behavior different in kind from that produced in a motivated state. This basic separation between motivated and frustrated behavior is in contrast to the view which postulates that all behavior has a motive. When it is assumed that all behavior is motivated, it follows that any behavior expressed is a *means* to some *end*. Thus, one is led to assume that if a child steals he is doing it to achieve some goal, or end. It is said that he is solving the problem of satisfying his wants or needs, even though he may be going contrary to some other needs, such as being accepted by society. From this point of view it follows that if we make stealing unattractive (punishing for the act), such behavior will be deterred. If, on the other hand, we recognize that there are two different kinds of behavior, then it follows that there may be two kinds of stealing, one that is motivated and solves the problem of gratifying needs, and one that is frustration-instigated and compulsive in nature. The latter type of behavior solves no problem and has no goal to direct it. It may occur in children from broken homes in which the child has adequate spending money. This type of stealing may involve the theft of objects for which the child has no need or interest. Such behavior is similar to vandalism, in which objects are destroyed rather than taken. The fact that stealing increases with frustration indicates

that we must distinguish between the various forms of stealing. The separation of behavior into frustration-instigated and goal-motivated behavior permits just such a distinction.

Once we accept the belief that behavior produced under frustration follows different principles from behavior motivated by goals, we can reorganize our knowledge of the subject of frustration. For example, it is known that destructive (aggressive or hateful) behavior is associated with frustration and that a frustrated person attacks his enemy. This behavior may appear to be problem-solving in nature, but difficulty is encountered in explaining why people who are frustrated so often strike out at innocent bystanders. One can see how the destruction of one's enemies would achieve objectives, but the fact is that frustrated persons do not always express their hates in such a manner as to solve problems. Instead, they create more problems by their hateful behavior. Thus, frustrated parents abuse their children and rationalize that they are training them. The children return the hatred or direct it toward society through delinquent behavior.

It seems useless to probe for problems which hate behavior solves. Instead, our theory suggests that frustration produces hate, and the hatred is directed toward anything that is convenient or is in the individual's attention during his frustration. Some of the animal experiments show that the type of behavior expressed in frustration is determined by its availability to the individual rather than by its effectiveness.

Another form of behavior associated with frustration is that of regression, which represents a type of behavior more childish than the individual's level of development warrants. Thus, bed-wetting in an eight-year-old is a sign of regression. A child that has learned to walk may temporarily revert to creeping when frustrated. Believers in the theory that all behavior is motivated have difficulty in explaining such senseless regressive behaviors. What problem is solved by this type of behavior? Frequently, it is said that the child desires attention. The attention he receives from bed-wetting, however, may be a spanking and degradation. Is this activity solving a problem for the child, or is it aggravating a condition that is already bad? If, however, we assume that frustration produces regression and that this simplification of behavior is a direct result of frustration, then our problem is to seek the source of frustration. The child that regresses may feel rejected. Punishment makes him feel more rejected. On the other hand, love and understanding reduce the state of frustration. It then follows that a child is most likely to be cured if he is given treatment that reduces his frustration, and this is frequently what the practicing psychiatrist recommends. He suggests love and attention because they work. Nevertheless, from a motivation point of view, rewarding a bad response with love should strengthen it. Yet both aggressive and regressive behaviors are reduced when treated with understanding and love.

From our point of view it follows that the behavior expressed gives no clue as to what the frustrated individual needs. A child that is insecure may deveolp a form of ritualistic behavior and so show signs of fixation; he may whine excessively, wet the bed, and have difficulty in learning, thereby showing signs of regression; he may become destructive with toys or be a bully in school, thereby showing aggressive symptoms; or he may show behaviors that are combinations of fixation, regression, and aggression. Regardless of which behaviors are expressed, however, the underlying cause may be the same. If the insecure or rejected child is to be made to feel secure, therapy is achieved in the same way, regardless of the specific symptoms that a given child exhibits.

To show more clearly the difference between motivated and frustrated behavior, we have listed in Table 1 those characteristics of each that seem to be sufficiently common to warrant inclusion (although there may be many others).

The differences in behavior listed in the table are basic, and failure to make these distinctions seems only to lead to inconsistencies and confusion. If these differences are recognized it means that the

TABLE 1

CHARACTERISTICS OF MOTIVATED AND FRUSTRATED BEHAVIOR

Motivation-induced	Frustration-instigated
Goal-oriented	Not directed toward a goal
Tensions reduced when goal is reached	Tensions reduced when behavior is expressed, but increased if behavior leads to more frustration
Punishment deters action	Punishment aggravates state of frustration
Behavior shows variability and resourcefulness in a problem situation	Behavior is stereotyped and rigid
Behavior is constructive	Behavior is nonconstructive or destructive
Behavior reflects choices influenced by consequences	Behavior is compulsive
Learning proceeds and makes for development and maturity	Learning is blocked and behavior regresses

first step in diagnosis is to determine which condition an individual is in when one attempts to correct behavior. The nonfrustrated person is subject to training because he is responsive to training methods, and he can be attracted to substitute goals.

The frustrated individual, however, needs relief from frustration. Can the situation be corrected? If so, then such correction is a form of therapy. Another possibility is to treat the *individual* rather than the *situation*. Can the state of frustration be relieved without making it necessary to change the situation? Actually the expression of a frustrated response reduces the state of frustration. An act of aggression such as writing a hateful letter achieves relief even if the letter is not mailed. Crying (a regressive response to frustration) reduces frustration and the person need not receive the concessions that tears sometimes attain. Rats which showed tendencies to have seizures when frustrated, had fewer seizures when they developed fixations. Thus the various frustration-induced responses seem to relieve the state of frustration, but it must not be supposed that the anticipation of such relief is an essential cause of the behavior. To make this supposition would deny the basic evidence which differentiates motivated and frustration-instigated behavior.

Unfortunately, the expression of frustration-instigated responses frequently leads to further frustration. When one strikes another or verbally abuses him, the other person strikes back and so creates a further problem. Thus the value of the relief gained through expression is offset by the fact that the end the expression has served is one which leads to new frustration. It is for this reason that therapy must permit harmless forms of aggression. Such harmless forms of aggression are encouraged in play therapy and in counselling situations. These permit children and adults to express hostility without having the behavior challenged.

We thus find that the experimentation with animals leads us to a theory of frustration which reorganizes the facts of human behavior and reinterprets the meaning and importance of certain forms of therapy. It has supplied us with certain basic principles which have a firm foundation in that the principles are experimentally derived. Whether the experimentation can proceed to aid us in answering many of our perplexing problems remains to be seen.

Certainly the world situation offers many problems. To what extent are we reading motives into the frustrated behavior of nations with which we come in conflict? To what extent are our behaviors frustrated reactions rather than problem-solving reactions? Can a frustrated world solve its problems? If therapy is needed, who or where can one find a disinterested party that can make the diagnosis? The frustrated person rationalizes and justifies his feelings and actions, and for this reason the patient cannot easily treat himself. Is this also true of the behavior of nations?

MALARIAL PARASITES AND THEIR MODE OF LIFE*

QUENTIN M. GEIMAN and RALPH W. McKEE

Dr. Geiman, of the Department of Comparative Pathology and Tropical Medicine, and Dr. McKee, of the Department of Biological Chemistry, Harvard Medical School, were the recipients of half the Annual Thousand Dollar AAAS Prize in 1946.

WIDESPREAD sickness and death in warm climates, puzzling riddles, invention and discovery, insight and synthesis, have carved the milestones marking more than two thousand years in the history of malariology. Beginning with the recognizable description of malarial fevers by Hippocrates, and "The Agues and Fevers that plagued our land" in *The Wasps*, by Aristophanes, countless references have been made in popular and scientific writing to one of man's worst infections.

No effort will be made to chronicle the relation of the discovery of the microscope by Leeuwenhoek to the first view of plasmodia in 1880 by the military surgeon Laveran; to demonstrate the part played by the development of polychrome stains in the discovery of the mosquito cycle of malarial parasites by Ross; to relate the explanation of the malarial paroxysm by Golgi to advances in pathology and haematology; or to show how chance led to the discovery of quinine as an antimalarial agent which was man's sole weapon against the disease for almost three hundred years. Instead, this account will deal with the application of closely and distantly related disciplines of knowledge to the study of plasmodia. Methods and results from the fields of nutrition and metabolism of cells and microorganisms are now being applied to studies of malarial parasites. Biochemical, clinical, and cultural procedures are shedding new light on the mode of action of antimalarial drugs. The studies and results to be mentioned have been made with certain species of avian, simian, or human malarias.

MALARIAL infections in man are caused by four species of parasites, *Plasmodium falciparum* of malignant tertian malaria, *Plasmodium vivax* of benign tertian malaria, *P. malariae* of quartan malaria, and *P. ovale* of *ovale* tertian malaria.[1]

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Superscript figures refer to sources omitted here owing to lack of space. References will appear in the reprints.

These human infections have certain features in common, such as mode of transmission by varying species of anopheline mosquitoes, parasitization of red cells, anemia, and the production of chills and fever characteristic of clinical malaria. However, each type of infection has distinctive biological characteristics and response to chemotherapeutic agents.

Malignant tertian malaria, a truly tropical infection of man, is responsible for the greater number of deaths. This parasite, in the primary infection, multiplies rapidly within the host red blood cells, with a periodicity of 48 hours, and produces an overwhelming parasitemia which can infect up to 50 percent of the circulating erythrocytes. During the growth of the parasites in the red cells, the membranes of the host cells are altered physically and chemically so that they clog the small capillaries and rob the surrounding tissues of nutrients and oxygen, resulting in cessation of the function of the involved organ. If the parasites localize in the brain, coma and death can occur within 3–5 days after the onset of the disease.

Benign tertian malaria is the type of infection with 48-hour periodicity which seldom kills. This type of disease is the cause of extensive morbidity because of clinical relapses and of the great difficulty in obtaining radical cures with available antimalarial drugs. The tissue stages apparently are suppressed but not killed by the majority of antimalarial drugs, emerging to produce relapsing malaria when the concentration of the drugs in the blood falls below effective levels.

Quartan malaria is produced by parasites with a cycle in the blood stream which requires 72 hours for completion. This infection can become latent and persist as long as forty years without causing clinical malaria in the interim. Evidence for this latency has been obtained from numerous infections of quartan malaria developing in recipients who had had direct transfusions from a pooled blood bank provided by donors with a previous history of malaria, usually in childhood.

Although experimental human infections in volunteers or in paretics receiving malaria for therapeutic purposes have provided extensive informa-

tion about the pathogenesis, immunology, and chemotherapy of the disease, studies with avian and simian infections have advanced our knowledge also. Avian species, *P. gallinaceum, P. lophurae,* and *P. cathemerium,* have been used experimentally, and studies with the simian species *P. knowlesi, P. cynomolgi,* and *P. inui* have furnished basic fundamental knowledge of the latter group. Since *P. knowlesi* has a 24-hour asexual cycle, is highly pathogenic for monkeys, and will produce clinical malaria in man, this species has been used for experiments by numerous investigators.

Prior to the widespread use of malaria for the therapeutic treatment of paresis, the discovery of the simian parasite *P. knowlesi,* and extensive studies of avian plasmodia, our knowledge of factors underlying fundamental host-parasite phenomena was very limited. The advent of experimental infections that could be studied and controlled under laboratory conditions provided the tools to fill the many gaps in our knowledge about the life cycle of the parasites and their biochemistry, immunity, physiological pathology, and chemotherapy.

Life cycle of parasites. Careful and thorough work by Huff and Coulston[2] led to the discovery of tissue stages of avian malarial parasites which develop in the cells of the host between the bite of the mosquito and their appearance in the circulating blood. This discovery provided new facts and evidence for the discovery of similar stages in human malarias. The work of Fairley *et al.*[3] showed that tissue stages existed in human malarias (*P. vivax* and *P. falciparum*). These stages have now been found in parenchymatous cells of the liver for two mammalian plasmodia, *P. cynomolgi* of monkeys and *P. vivax* of man.[4] Since the relapsing nature of human malarias, particularly *vivax* malaria, is related to the existence and persistence of these tissue stages, methods and techniques will be sought to study the effects of drugs. This information will also be helpful in finding more effective and less toxic drugs to produce radical cures.

Immunity. The newer knowledge of immune phenomena in malaria and of the relation of immunity to treatment has provided explanations for the nature of acquired immunity, and the concepts affecting the use of clinical or radical cures in achieving malaria control in endemic and hyperendemic malarious areas.[5, 6, 7, 8] The reasons for the low-grade antibody production to antigens derived from asexual stages of plasmodia and the precise metabolic effects of antibodies on growth and multiplication of the parasites have not been studied.

Physiological pathology. Malaria became an acute problem in World War II. Prior to the war clinical and experimental evidence partially explained the pathogenesis of malarial parasites. Experiences with large numbers of infections contracted in malarious areas during operations in the war emphasized the extent of unsolved problems. The acute and often fatal termination of *falciparum* malaria and the serious relapsing nature of *vivax* malaria stimulated more thorough investigations into the cause of death and the extent of disability caused by attacks of the disease.

In spite of the reinvestigation into the mechanism of death in acute malarial infections, controversy still exists. The sequence of events leading up to death in acute monkey infections with *P. knowlesi* has been studied in vivo by Knisely *et al.*[9] Explanation by these authors of "sludged blood" leading to stasis of the circulation, anoxia, and death in acute malaria is interpreted somewhat differently by Rigdon.[10] The rapid destruction of red cells by the parasites and by phagocytes is considered as the primary cause of anoxia and hence of the lesions found in cerebral and visceral involvement.

The investigations conducted by the Army and Navy [11, 12] to discover the organic and psychological causes for morbidity produced by *P. vivax* have added needed information about this infection and its sequelae. The chronic symptomatology which was found was attributed primarily to the malaria. On the other hand, many of the so-called effects ascribed to malaria arose from the personality of the individual and not from the disease.

These studies are of prime importance to the clinician and the pathologist. An accumulation and elaboration of biochemical and nutritional data about plasmodia should eventually lead to an understanding of the underlying ability of malarial parasites to produce disease and death.

Biochemistry. A substantial series of studies on the biochemistry and metabolism of malarial parasites has been made during the past decade. Rapid strides in biochemistry have provided essential methods and techniques applicable to studies of bacterial and parasitic metabolism and nutrition. Microorganisms are not simple in their structure and metabolism but highly complex in their cellular make-up and life processes.[13] The protozoa which cause malaria require diverse physical and chemical conditions for their existence and transmission from mosquito to man. Little is known about the requirements for development of plasmodia in anopheline mosquitoes. Furthermore, the invasion and growth of the asexual

stages in erythrocytes further complicates studies of the requirements for growth, multiplication, and mechanism of disease production.

The first basic metabolic studies on plasmodia were made by Christophers and Fulton in 1938.[14] Prior to that time it was known that parasitized blood required glucose and that such blood became "blue" after standing in a test tube, owing to the removal of oxygen by the parasites. These British workers studied a number of carbohydrate nutrients and found that glucose was oxidized by the parasite *P. knowlesi*. In addition, they found that blood from infected animals consumed oxygen to produce carbon dioxide much more rapidly than normal blood.

In 1941, Maier and Coggershall[15] extended the experiments to demonstrate that various carbohydrates were capable of maintaining the oxygen uptake of *P. knowlesi, P. inui,* and *P. cynomolgi* of monkeys, and *P. cathemerium* and *P. lophurae* of birds. An attempt was made to correlate the inhibitory action of drugs on oxygen uptake to antimalarial activity, but the correlation was not clear-cut.[16] In the canary infection, *P. cathemerium,* Velick[17] correlated growth and nuclear division of the parasites with increased respiration, respiratory quotient, and cytochrome oxidase activity. Wendel[18] greatly extended our knowledge of the metabolism of the malarial parasite *P. knowlesi.* Respiratory activity with glucose and lactate substrates was equally good. Wendel confirmed the earlier work showing that glucose and oxygen are utilized with great rapidity by parasitized cells and that lactic acid accumulates because only about half the formed lactate is oxidized. Additional glucose is rapidly converted by the parasites to lactic acid, producing a rapid fall in pH and a subsequent drop in respiration.

In early morphological studies of plasmodia in red cells, it was observed that the host cell became increasingly pale as the parasite grew within. This observation suggested the utilization of hemoglobin by the parasites to form the pigment laid down in the cytoplasm. Brown[19] showed that hemoglobin of the host erythrocyte was split to form hematin, but the fate of the globin portion of the molecule was not known. More recently, the work of Moulder and Evans[20] and of Morrison and Jeskey[21] has shown that the protein of the red cell is hydrolyzed and about half the amino acids are utilized by the parasite for the synthesis of protein, the remainder of the amino acids dialyzing out of the cell.

This brief review indicates the basis for the biochemical and cultural studies begun by our group at the Harvard Medical School in 1943. The basic nature of the early work and the method of approach to unsolved problems were aptly stated by Wendel:

> . . . knowledge of the metabolic characteristics of malarial parasites may lead to an understanding of the lethal action of quinine, atebrin and plasmochin on *Plasmodia*. Such knowledge may, perhaps, simplify search for other antimalarial agents. Short of this, chemical studies should reveal the nutritional requirements and end-products of the metabolism of malarial parasites, in consequence of which *in vitro* cultivation may be feasible.

Extensive information had accumulated between the discovery of plasmodia by Laveran in 1880 and 1943, but fundamental biochemical data was meager and the requirements for in vitro cultivation were only partially defined.[22, 23]

Since the production of clinical malaria in man involved the asexual cycle of the parasite in the erythrocytes floating in the plasma, more chemical knowledge was needed about the erythrocyte and the plasma. *P. knowlesi* of monkeys was chosen for the basic studies, [24, 25, 26] and analysis of the major inorganic components of the blood (*Macaca mulatta*) was undertaken. A comparison of normal and parasitized blood gave similar results with the exception of an elevation of plasma potassium for a short time following rupture of parasitized red cells and a slightly lowered blood inorganic phosphorus, which is due to the conversion to organic phosphorus by the parasites.

Simultaneously with this attack, extended metabolic and chemical studies were made both on normal and infected erythrocytes. Many of the results of earlier workers with *P. knowlesi* were confirmed, namely, the maintenance of respiration with glucose or lactate, and the increased respiration with the increase in size of the parasite. In addition, glycerol was shown to be even more effective than glucose or lactate in the respiration of the parasites. Amino acids as a substrate produced a small uptake of oxygen, whereas succinate and acetate appeared to have no effect. Respiration was shown to be inhibited by cyanide, carbon monoxide, and high oxygen tensions. It was found that the parasitized erythrocyte converts glucose to lactate at a rate 25–75 times that of the normal monkey erythrocyte—increasing as the parasite grows. However, the lactate that is formed is oxidized at only one sixth to one third the rate at which it is produced. Chemical analyses of normal and parasitized cells for fatty acids and total and partitioned phosphorus showed that each parasitized cell contains 4 times the fatty acids, 3 times the total phosphorus, 2.4 times the 15-

minute hydrolyzable phosphorus (approximately two thirds of the adenosine triphosphate phosphorus), 3.8 times the phospholipid phosphorus, and 13.5 times the nucleic acid phosphorus found in a normal red cell.

This ability of parasitized erythrocytes to convert glucose to lactate and to oxidize lactate and pyruvate indicates that the malarial parasite synthesizes both glycolytic and oxidative enzymes. In view of this a study was made of the lactic dehydrogenase enzyme and of the flavine adenine dinucelotide content of both normal and parasitized red cells. Two to 2.5 times more of the dehydrogenase and 9 times more flavine adenine dinucleotide are present in parasitized than in normal cells.[27]

Speck, Moulder, and Evans,[28] studying *P. gallinaceum*, determined both the glycolytic cycle and the oxidative systems (tricarboxylic acid cycle) involved in the parasite. They also obtained evidence linking protein metabolism to the oxidative processes.

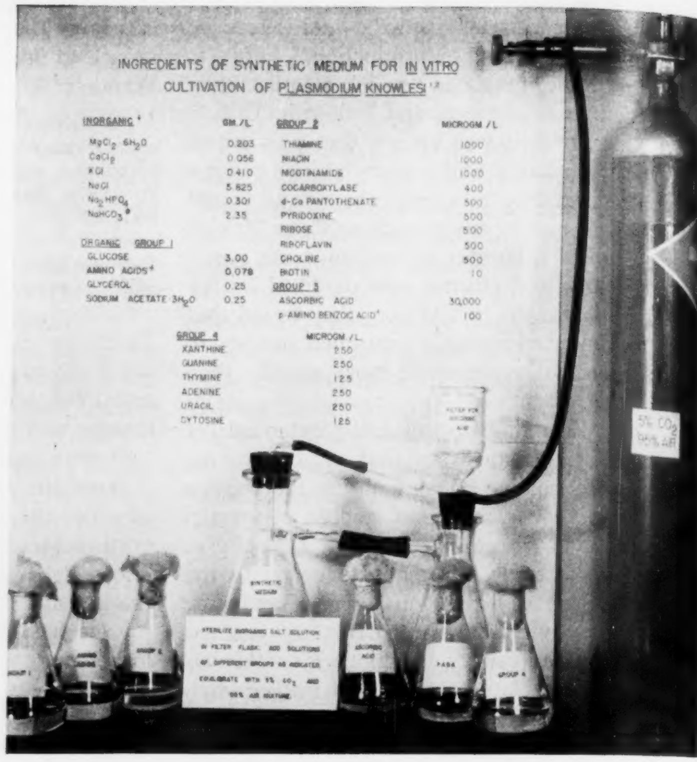


Fig. 1. To make the synthetic medium for the cultivation of mammalian malarial parasites, stock solutions of the different "blocks" of nutrients are combined.[26]

Bovarnick, Lindsay, and Hellerman,[29] studied the glycolytic and oxidative processes involved in *P. lophurae*. They have shown that succinate and fumarate, components of the tricarboxylic acid cycle, produce oxygen uptake which is inhibited by cyanide and by atabrine and quinine. These antimalarials also inhibit the phosphorylation of glucose.

As stated previously, glycerol is oxidized by the erythrocyte parasitized with *P. knowlesi* even more avidly than is either glucose or lactate. This is true also for *P. cynomolgi* and *P. vivax*.[30] This rapid oxidation of glycerol is of considerable interest because, first, it undoubtedly indicates the presence of an additional oxidative enzyme system; and, second, although glycerol is effective as a respiratory substrate, it will not act as the chief caloric source in maintaining the growth of parasites. The presence of glucose is absolutely essential for normal in vitro growth of parasites in all the systems tested thus far.

The great rapidity with which glucose is converted to lactic acid, with its consequent accumulation owing to the oxidation of only a fraction of the total lactate formed, causes a tremendous accumulation of acid. It is for this reason that either dilution or perfusion methods had to be utilized for the in vitro cultivation of the malarial parasites.[25] The dilution procedure involves both limiting the numbers of parasites and adding to the parasitized blood three volumes of nutrient fluid (Fig. 1) so that the accumulating acid is adequately diluted. The perfusion method allows for the outward diffusion of lactate, other waste products, and cell and plasma components not present in the nutrient medium, with the inward diffusion of substrates. Both these procedures (Fig. 2) have been utilized successfully for the nutritional and cultural studies—each having certain advantages and certain limitations. In vitro growth and multiplication have been obtained with five species of malarial parasites, *P. knowlesi*, *P. cynomolgi*, *P. lophurae*, *P. vivax* (Fig. 3), and *P. falciparum*.[30] To obtain quantitative data about the parasites before and after culture, films are made on cover glasses for staining, counting, and evaluation, and erythrocyte counts are obtained.[25] The amounts of glucose and lactate utilized are an index of the in vitro growth of the parasites.[24]

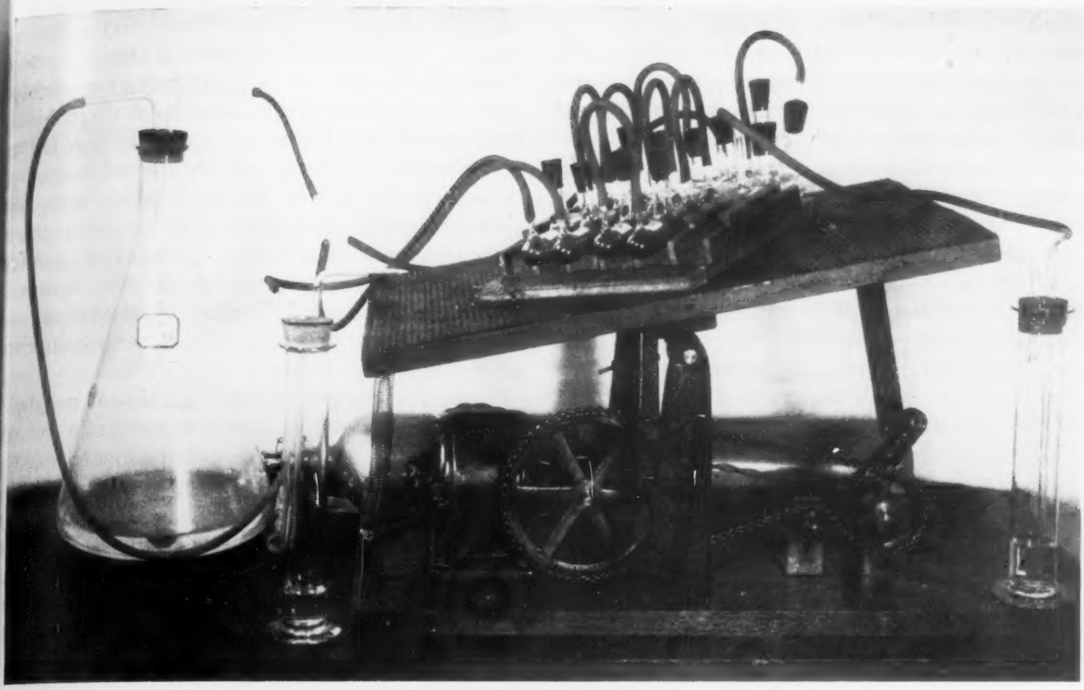


Fig. 2. Type of rocker used to agitate parasitized blood being cultivated in a warm room. The pH of the cultures is maintained by passage of a mixture of 5 percent carbon dioxide and 95 percent air through the vessels.[34]

Parasitological and biochemical data are combined to interpret the results of experiments.

The in vitro studies have shown that an isotonic inorganic medium, similar in composition to the host's plasma, is essential for the maintenance of red cells and for growth and multiplication of malarial parasites.[24] In addition, glucose is essential as a source of energy, *p*-aminobenzoic acid (PABA) as a growth accessory, and amino acids for protein synthesis. Vitamins, and purines and pyrimidines, both added as "blocks," are important in the nutrition of *P. knowlesi*.[26]

In attempting to evaluate further the essential or accessory nature of other nutrients in the medium for growth and multiplication with the dilution method, masking effects due to the presence of plasma prevented interpretation of results.[26] A dilution technique was then devised utilizing normal parasitized erythrocytes freed of plasma by washing and subsequently suspended in the isotonic culture medium.[30] With such a system, little if any growth and multiplication of parasites could be obtained unless a protein was added to the nutrient medium. Purified albumins and globulins from both human and bovine plasmas have been used with success. A system containing crystalline bovine albumin as a source of protein has been utilized in the re-evalution of the inorganic com-

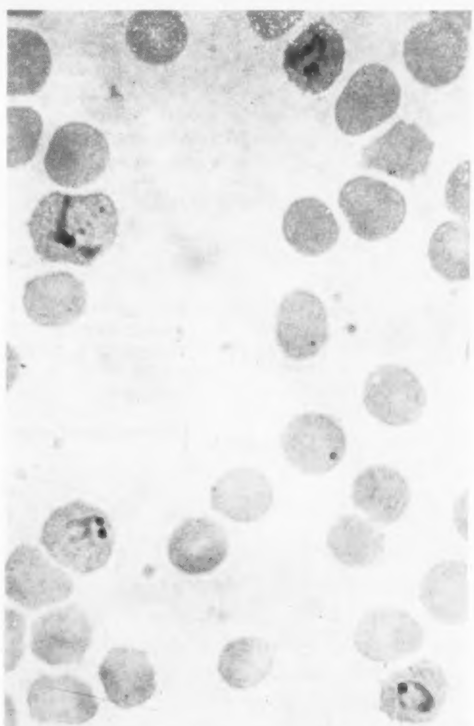


Fig. 3. Photomicrograph of a third in vitro generation of *P. vivax*. This blood film was made 68 hours after the start of the culture. Mag. × 1,100.

ponents of the medium, pH and tonicity, the individual amino acids, glycerol and sodium acetate, ascorbic acid, certain vitamins, purines and pyrimidines, and known plasma substances not present in the nutrient medium.[31]

The most striking effect observed in these recent in vitro studies was the essential need for methionine.[32, 33] If methionine is absent from the environment of the red cell, very poor growth and multiplication occur. The addition of albumin to the plasma-free medium had no influence on the amino acid requirement—methionine was still essential. This apparently means that proteolytic enzymes are not present outside the erythrocyte; otherwise, adequate methionine would be obtained from the hydrolysis of the albumin. The need for added methionine is not too surprising in view of the fact that hemoglobin contains only about 1 percent methionine, whereas most proteins—and possibly parasite protein—contain 3 or 4 times that quantity.

In spite of the dramatic effects produced by the additions of protein and methionine to the nutrient medium used in this washed-cell culture system, there still remain one or more factors needed for optimal growth and multiplication of the parasites, both simian and human. These unknown factors are present in plasma and are diffusible through cellophane, as shown by experiments using the perfusion culture technique. In an attempt to determine these unknown factors, a number of materials have been tested, none of which show beneficial results. In this regard, Trager[22] showed that a red cell extract was beneficial to the in vitro survival of *P. lophurae*.

The ultimate goal in nutritional and metabolic studies of malarial parasites is to obtain growth and multiplication of the organisms in a completely synthetic medium and devoid of the host red cell.

In Figure 4 an effort has been made to outline diagrammatically the growth and metabolism of the asexual stages of plasmodia. The chart was compiled from the results of metabolic and nutritional studies carried out in various laboratories

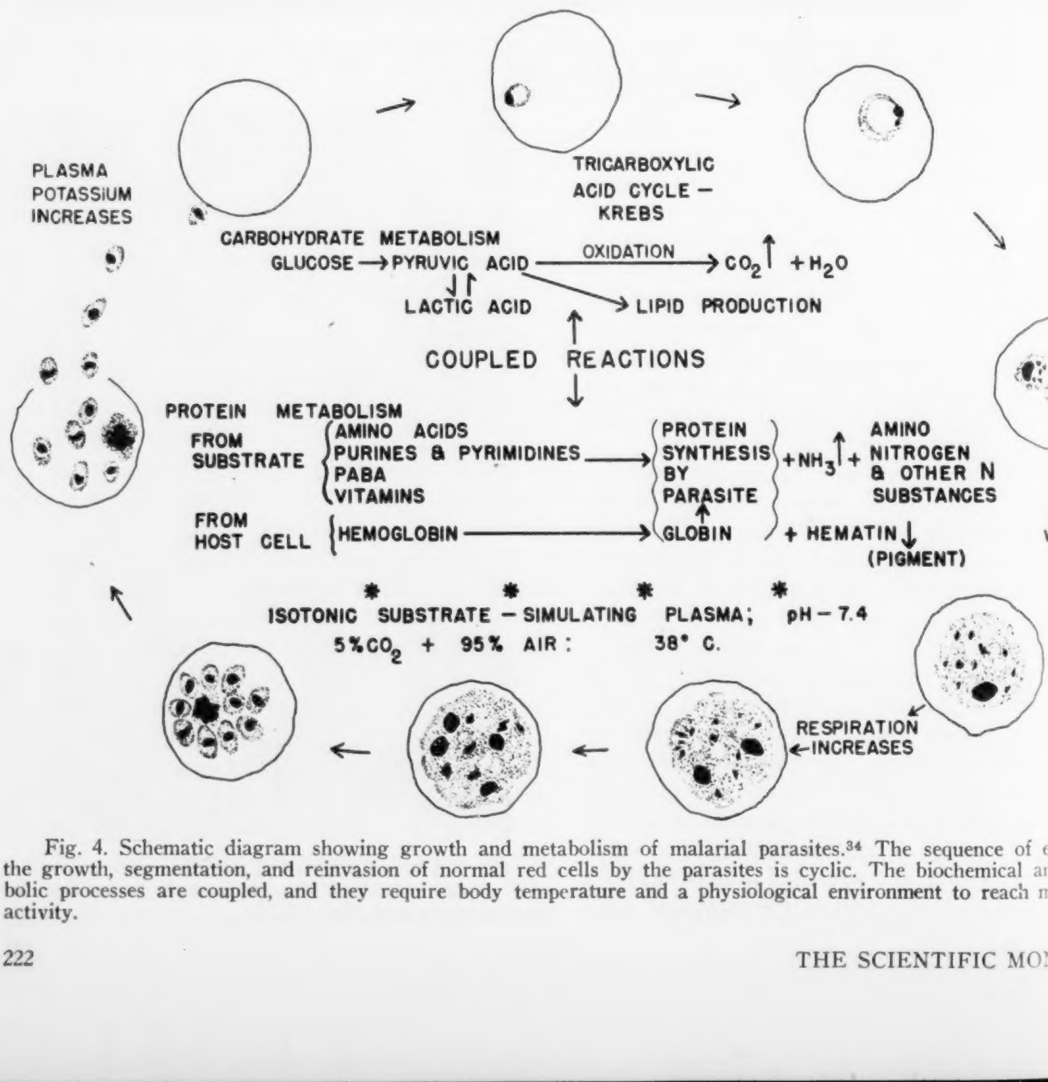


Fig. 4. Schematic diagram showing growth and metabolism of malarial parasites.[34] The sequence of events in the growth, segmentation, and reinvasion of normal red cells by the parasites is cyclic. The biochemical and metabolic processes are coupled, and they require body temperature and a physiological environment to reach maximum activity.

and discussed elsewhere.[34] It is sufficient to add, and to emphasize, that our knowledge is far from complete, and that more studies are needed to define further the nutrition and metabolism of the parasites and the properties of the red cell so necessary for the life of asexual stages of plasmodia.

Considering briefly the in vitro requirements of malarial parasites, we have found recently that the nutrition of the monkey (*Macaca mulatta*) is of great importance to the course of infection with *P. knowlesi*. Monkeys that were deficient in ascorbic acid, either naturally occurring or artificially produced, grew only a limited number of parasites.[35] The usual highly pathogenic course of infection was restored by the administration of pure ascorbic acid. A more recent finding, and one which we believe to be of even greater interest and importance, is the control of parasitemia by withholding food from infected animals (Fig. 5). This control of parasitemia can be reversed if a full diet or if only methionine or *p*-aminobenzoic acid is administered. However, the inhibition of parasite growth is not released if food is given about 8 or 9 days after inoculation with parasites. Immune mechanisms aid in controlling parasitemia at that stage of the infection even though the restored diet is adequate. In other words, adequate nutrients must be present in the animal to grow parasites. If the rate of parasite multiplication is retarded below a certain level, immune mechanisms are mobilized to control the further growth and multiplication of parasites. These observations have many applications to practical malarial problems and need to be explored further.

NUMEROUS free-living protozoa[36] and bacteria[37] have comparatively simple nutritional and cultural requirements. Tissue cells,[38] parasitic protozoa, and numerous species of pathogenic bacteria are more fastidious in their requirements. In most cases, media for cultivation were devised empirically and, when successful, answered laboratory requirements for study and propagation of the organisms. Similar attempts to devise methods and cultural procedures for asexual stages of malarial parasites were impeded by the complexity of conditions which had to be met, and the complicated problem of maintaining blood in vitro in a physiological state. The accumulation of biochemical and nutritional knowledge about plasmodia is necessarily dependent on, and limited by, the extent and advance of our knowledge about blood.

The primary environment of malaria parasites is blood. Blood plasma and red cells contain complex mixtures of inorganic and organic chemicals, with specific physical and chemical properties delicately balanced by enzyme- and hormone-regulating mechanisms according to the specific host. The blood components vary with the nutrition of the host and the function and secretion of the organs in the body. The specific biochemical dif-

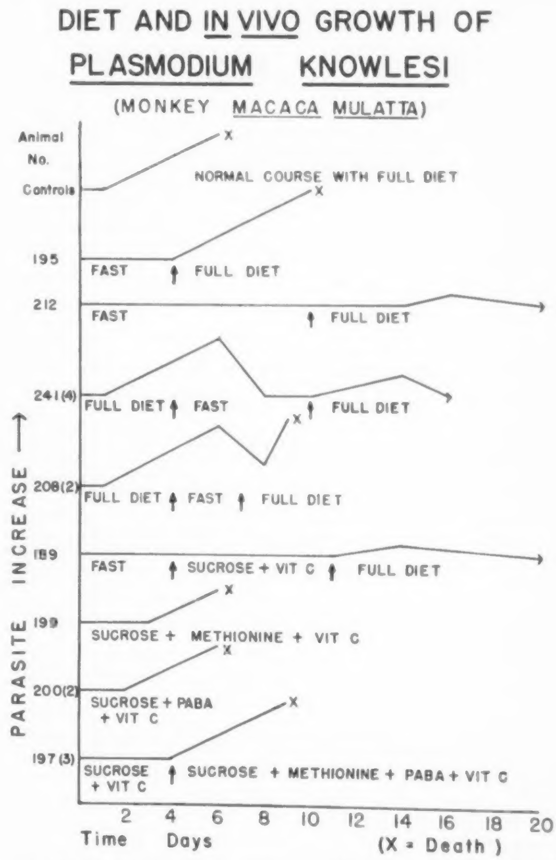


Fig. 5. Schematic representation of the control of parasitemia in monkeys as a result of the withholding of food, of the release of this control upon giving a full diet, or upon the administration of methionine and PABA. It also indicates the counteraction of antibody proteins in the plasma on parasite growth about 9 days after parasite inoculation.[33]

ferences between the blood and tissues of different hosts are providing new concepts in the study of host-parasite relationships.

Since previous empirical efforts failed to produce satisfactory in vitro methods for the propagation of plasmodia, it became necessary to approach the problems systematically by analyzing host plasma and the properties of plasmodia in order to provide a foundation on which to build. Reference to previous papers and to the partial discussion here indicates the elaborate methods and techniques that have been found necessary to achieve growth and multiplication of malarial parasites. It is hoped,

however, that the procedures can be simplified later.

Whether simplification of methods and the synthetic medium now in use will be possible is not known at this time. The requisite components and properties of the synthetic medium for cultivation are determined by the dual need of supporting the host red cell and its parasite in a physiological state for growth and multiplication. The nutrition of the parasites involves the splitting and utilization of hemoglobin, to form pigment (hematin) and to synthesize parasite protein, and the availability of diffusible substrates from the nutrient medium. Although the parasite contains enzyme systems, nothing is known about the supporting role which the host cell plays either in the nutrition of the parasite or in supplying a physical intracellular matrix needed for the existence of the parasite.

Unsuccessful attempts to cultivate the parasites free of the red cell in media simulating the intracellular components of the erythrocyte suggest the existence of unknown requirements for growth. Precise reasons for the immediate reinvasion of normal red cells by the progeny or merozoites of each asexual generation are not known. Perhaps the erythrocyte provides a more compatible inorganic and organic environment to carry on metabolic functions. When available, a comparison of the growth requirements of other obligate intracellular parasites, such as *Rickettsia* and *Toxoplasma* with those of *Plasmodia* will be of great theoretical and practical value.

The competition of the malarial parasite with cells and tissues of the host for nutrients forms the basis for host susceptibility, virulence, intensity or levels of parasitemia, and stimuli for the initiation of immune reactions. The ability of the host to provide the specific metabolites for the parasites determines the initial rate of growth and multiplication. Natural or experimental interference with any of the key metabolites (Fig. 4) will affect one or all of the categories of host-parasite relationships. Chemotherapeutic interference to produce clinical or radial cures of the infection kills the parasites by direct action or selectively inhibits vital enzyme systems.

Thus, factors which influence host-parasite relationships and susceptibility to plasmodia can now be analyzed in vitro and in vivo. The results will aid in evaluating the mode of life of malarial parasites and when applied to other parasitic protozoa should provide needed information of value in combating important human infections.

BIOLUMINESCENCE: A REACTION RATE TOOL

FRANK H. JOHNSON

Professor Johnson, Department of Biology, Princeton University, shared the Annual Thousand Dollar AAAS Prize in 1941 with Drs. D. E. S. Brown and D. A. Marsland.

The stars flame down . . . and who will say
This worm is not as great as they?
"Glowworm," by Daniel Smythe

THE problem of reaction rates in biological processes is the most complex and at the same time the most fascinating of all kinetic phenomena. How is it, for example, that among mammals a Blue whale, say, may be born with a weight of some 7,000 pounds whereas a human baby weighs only 6 or 8 pounds? Yet both begin their individual life as a single microscopic fertilized egg, and the period of gestation is approximately the same. In another three years, the whale has reached maturity, sometimes attaining a length of 100 feet and weighing more than 200,000 pounds. This is an impressive example of normal differences in the chemical reaction rates responsible for growth, at practically the same constant temperature, among related organisms. Such differences are, of course, under genetic control, but a recognition of this fact merely poses one aspect of the problem to be explained. Moreover, the influence of the genes may be modified, and to some extent overridden, by influences of the environment. Thus, an excess of a single protein, the growth hormone of the anterior pituitary, leads to abnormally large but symmetrical growth of a maturing individual, whereas a deficiency of the same substance results in a dwarf; a lack of vitamin A impairs vision; and insufficient oxidation of phenyl pyruvic acid in the body is always correlated with a certain type of idiocy.

Familiar examples of the significance of reaction rates in living organisms are legion. Cancer is one of the most important of the unsolved problems. In leukemia, a cancerous state of the white blood cells, the population of leucocytes becomes abnormally large. Normally, a subtle control of relative reaction rates maintains a delicate balance between the rate of formation and rate of destruction of these cells. The balance may be upset not only by cancer, but also by a bacterial invasion, whereupon the leucocytes increase to several times their normal population. Perhaps in both cases an inhibitory factor is removed. In any case, whether we deal with growth, reproduction, longevity, respiration, or other complex phenomena, including the thought process itself, inhibition as well as acceleration are fundamental to the normal rate control of the numerous reactions, simultaneous as well as consecutive, which lead to the observed result.

Any attempt to analyze the mechanism of biological reaction rate control must first take into account this fact—axiomatic among biologists—that the reactions are enormously complex. A second fact, paradoxical though it seems, is that, under given conditions, in a given genetic environment, a whole complex process may behave much in the manner of a single chemical reaction. The scientific literature is replete with instances where the rate increases over a considerable range of rise in temperature, in accordance with the Arrhenius equation, a mathematical relation that was originally discovered through studies on the rate of hydrolysis of sucrose by various acids. The usual interpretation is that in such cases the total rate is limited by the velocity of the slowest reaction involved, e.g., the activity of a single protein catalyst, or enzyme. Unlike most ordinary chemical reactions, however, the biological rate does not continue to increase more or less indefinitely as the temperature is raised. It always reaches a maximum, the so-called optimum, then decreases. Thus, at the higher temperatures, a different reaction becomes important in limiting the rate of the over-all process, and there are substantial grounds for believing that this reaction is the thermal inactivation of one or more of the enzymes concerned.

The activity of a critical enzyme with its substrate may determine the rate of a complex biological process. As an approach to the analysis of factors which influence the total rate, many processes, including growth, are too complicated, or the experiments too time-consuming, to provide favorable material for study. Under different conditions, different systems might assume control of the rate. Ideally, one would like to have some process whose rate is limited largely by the same enzyme system under a wide variety of conditions. Furthermore, one would like to have a means of measuring readily and accurately the activity of that system. Finally, if it is not asking too much,

one would like to have some visible, instantaneous index to its activity, such as a little glow[1] of light whose brightness would be proportional to the reaction velocity of the system. A system with these characteristics would be an ideal tool.

Nature has been kind to the investigator of biological reactions in providing just such a system: bioluminescence. Nature has been kind, again, in bestowing this fascinating property upon bacteria, which are widely distributed in the sea, easily isolated, and conveniently cultivated in the laboratory. Moreover, in bacteria the luminescent reaction, under favorable conditions, is a steady-state process, the emitted light glowing at constant intensity over considerable periods of time. Yet it responds at once to alterations in temperature and hydrostatic pressure, as well as to the addition of many chemical agents of physiological and pharmacological interest which react directly with the light-emitting system. Luminescence responds immediately by changing to a faster or slower steady state, indicated by a different, constant brightness. Or, if the environmental conditions are so drastic as to destroy the system, the rate of destruction may be accurately followed by the progressive decrease in brightness, even when this rate is so fast that it could scarcely be measured by any other means.

With the unique advantages found in luminescence, it has been possible to analyze to an unusual extent the mechanism of factors which influence the rate of a biological process within a living cell, and the results have been of some general significance. Since the quantitative aspects have been set forth at length in papers by Johnson and collaborators, Eyring, Brown, and others, the discussion in this article deals primarily with the basic ideas in a largely qualitative manner.

The luminescent reaction. First, with regard to bioluminescence itself, the work chiefly of Harvey and his associates has shown that the brightness of this "cold light" is proportional to the reaction velocity of the oxidative enzyme luciferase with its substrate, luciferin. From certain organisms such as the ostracod *Cypridina,* these components may be extracted and partially purified. When aqueous solutions of the two are mixed together in the presence of molecular oxygen, an emission of light takes place (Figs. 1, 2), but it rapidly diminishes as the substrate is destroyed through nonluminescent reactions. In luminous bacteria a similar system is evidently concerned in light production, but is more intimately bound to the living cell (no attempts to extract it have been successful so far)

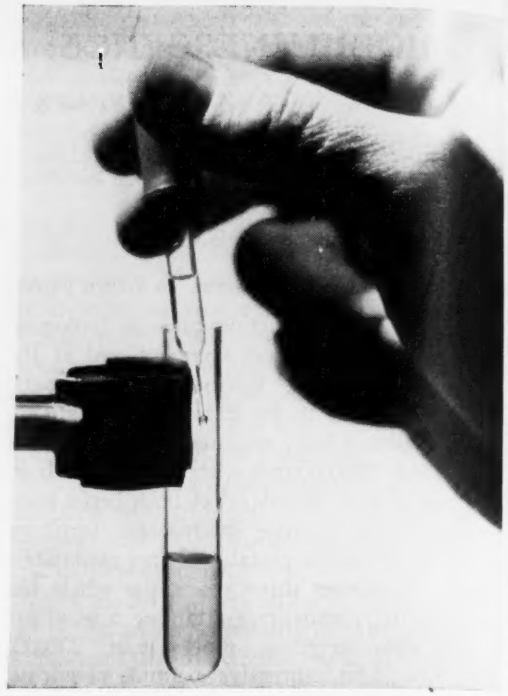


Fig. 1. Luciferin-luciferase reaction, with extracts of dried *Cypridina.* A partially purified solution of the enzyme luciferase is held in the small medicine dropper just before adding to solution of luciferin in test tube.

and, as already mentioned, may remain in a steady state for some time. Both materials are useful: evidence as to the site of action of various factors, such as drugs, can be obtained with purified extracts, and other kinetic aspects can be more easily studied with bacteria. Moreover, the latter are easier to obtain and prepare in abundance.

Most specimens of marine fish in any market have a few cells of luminous species of bacteria on their bodies. If the fish is placed in a shallow pan containing a 3 percent salt solution and incubated at 10°–20° C, colonies of these bacteria develop in a day or two and may spread over a large part of the body surface before putrefaction sets in (Fig. 3). Pure cultures are isolated by ordinary bacteriological procedures, using a culture medium containing 3 percent salt. For experiments, the cells are usually grown on agar until maximum brightness is attained, and then emulsified in buffered salt solution containing an excess of glucose as hydrogen donator, followed by thorough aeration with air or oxygen. Under these conditions, the rate of the luminescent oxidation is limited primarily by the activity of the luciferase.

The action of temperature and pressure. With the advent of Eyring's theory of absolute reaction

rates in 1935, the rational basis of chemical reaction rates became available in the expression

$$k' = \kappa \frac{kT}{h} K^{\ddagger} \qquad (1),$$

in which k' is the specific rate constant, and $K^{\ddagger}$ is the equilibrium constant between the reactants and the activated complex. Once formed, the activated complex decomposes with a universal frequency, $\frac{kT}{h}$, which is the same for all chemical reactions. In this term, k is the Boltzmann constant, T the absolute temperature, and h is Planck's constant. The transmission coefficient, κ, takes into account the probability that the activated complex, after breaking apart, will not immediately reconstitute itself. For most reactions the value of κ is very close to unity. All the methods of thermodynamics are applicable to the equilibrium constant $K^{\ddagger}$, exactly as they are to any ordinary equilibrium constant, the dagger superscript indicating "of activation" rather than "of reaction." Thus

$$-RTlnK^{\ddagger} = \Delta F^{\ddagger} = \Delta H^{\ddagger} - T \Delta S^{\ddagger} = \Delta E^{\ddagger} + p \Delta V^{\ddagger} - T \Delta S^{\ddagger} \qquad (2),$$

in which $\Delta F^{\ddagger}$ is the free energy of activation, i.e., the difference between the free energy of the activated complex and reactants; $\Delta H^{\ddagger}$, the heat of activation (corresponding to the "μ" of the Arrhenius equation); $\Delta E^{\ddagger}$, the energy of activation; $\Delta V^{\ddagger}$, the volume change of activation; $\Delta S^{\ddagger}$, the entropy of activation; p, the pressure; R, the gas constant; and T, the absolute temperature. It follows that, other things being constant, the reaction rate will be determined by temperature and pressure, in accordance with the values of $\Delta H^{\ddagger}$ and $\Delta V^{\ddagger}$, respectively. Numerical values of these constants may be calculated from a few data relating the rate of the reaction to the variables, temperature and pressure, respectively, and the rate can then be predicted for other values of the same variables than those measured, provided the mechanism of the reaction does not change.

In biology, the same theory applies to each individual reaction among those which are involved together in bringing about a complex process. This theory for change, along with the classical thermodynamic theory for ordinary equilibria, constitutes the ultimate basis for interpreting the control of the rate. Both have been applied quantitatively to bacterial luminescence and the whole process, under the specified conditions, found to behave much in the manner of a simple reaction.

First, with respect to temperature, when a test tube containing a suspension of luminous bacteria

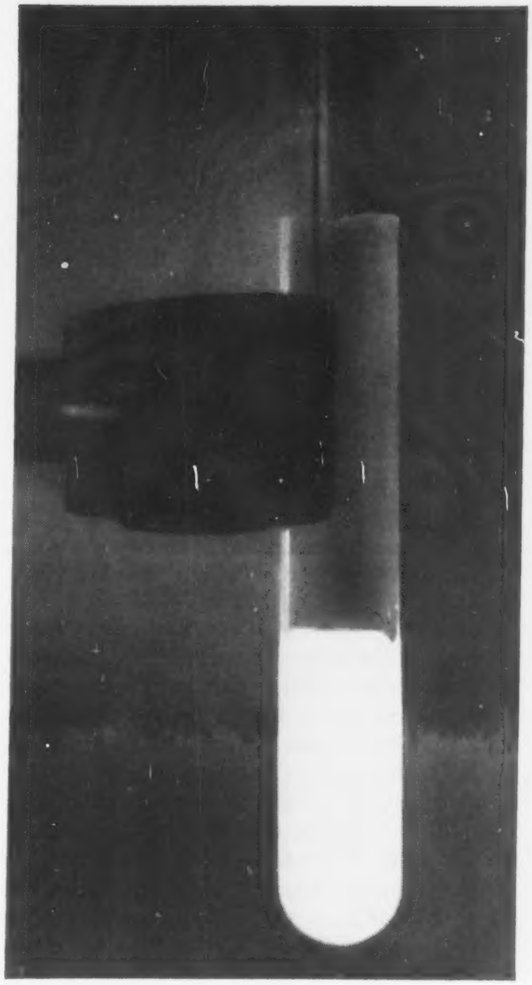


Fig. 2. Photograph of same tube taken by the cold light that was emitted as soon as the two solutions were mixed.

is placed in ice water, luminescence becomes relatively dim. If the tube is gradually warmed, the brightness increases progressively with rise in temperature, finally reaches a maximum, and then decreases with further rise in temperature until it all but disappears (Figs. 4, 5). At this point, if the tube is placed at once in the ice bath again, luminescence grows brighter, reaches its former maximum (Fig. 6), and decreases as the temperature of the cells changes from warm to cool.

This is a critical experiment, revealing a fact of basic importance, namely, that the heat- as well as the cold-diminution of the observed reaction is readily reversible. Two limiting reactions are involved in the control of luminescence, one predominant in the low temperature range, the other in the high. Quantitative analyses leave little

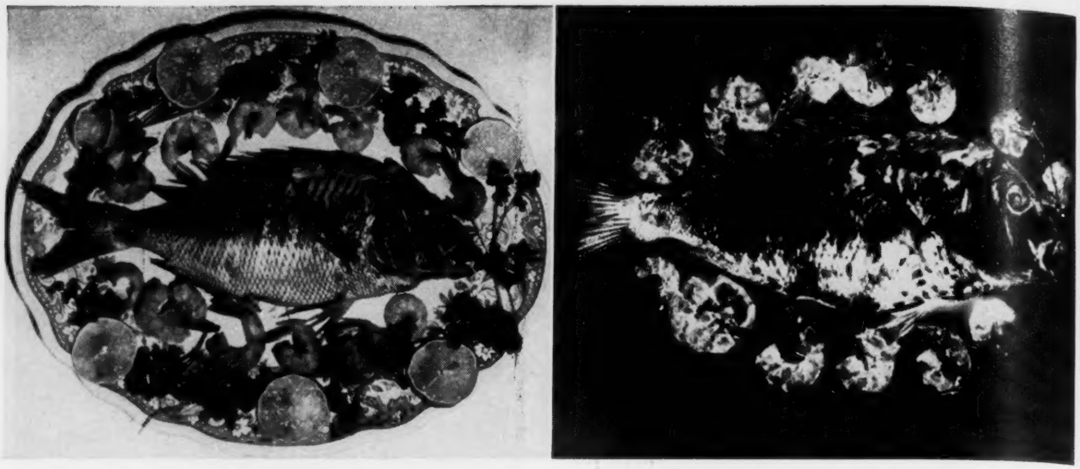


Fig. 3. *Left,* a fish dinner, with shrimp, appetizingly garnished with parsley and lemon; *right,* same specimens, as seen in the dark, both fish and shrimp glowing with the pale, blue-green light given off by nonputrefactive luminous bacteria growing on their su ace. A few generations ago, before modern refrigeration, it was not unusual to find uncooked meat and sea food exhibiting such an appearance. It was well known to Robert Boyle and many others as long ago as the seventeenth century.

doubt as to the nature of these reactions: the first is the rate of the catalytic, enzyme reaction itself, i.e., the luciferin-luciferase reaction which leads to the emission of visible light; and the second is a mobile equilibrium through which the active enzyme is denatured to an inactive form. The former has an apparent activation energy ($\Delta H^{\ddagger}$) of 15,000–20,000 calories, typical of biological oxidations and many enzyme reactions as well as more complex processes, whereas the latter has a heat (ΔH) of 55,000 or more calories and entropy (ΔS) of some 200 E. U., indicative of protein denaturation.

A second observation of basic importance concerns the influence of hydrostatic pressure. At low temperatures, the application of a few hundred atmospheres of hydrostatic pressure immediately reduces the brightness of luminescence. At the temperature of maximum brightness, however, such pressures have scarcely any observable effect, and at still higher temperatures, in the range where heat diminishes the brightness, pressure will at once counteract this diminution (Fig. 7). The opposite effects of pressure show that different reactions are predominantly in control of luminescence brightness at the two extremes of the temperature range. There is evidence that these reactions, again, are the enzyme reaction itself and the reversible denaturation of the enzyme, respectively. The former proceeds with a volume increase of activation ($\Delta V^{\ddagger}$) of some 50 cc/mole, whereas the denaturation is accompanied by an even larger volume increase of reaction (ΔV), closer to 100 cc/mole. Pressure thus retards the reaction most noticeably under conditions where the enzyme is essentially all in the native state, as at low temperatures. When, however, a considerable fraction of the enzyme is in the reversibly denatured state, the application of pressure shifts the equilibrium in favor of the active form and the intensity of the light increases. This effect arises from the larger volume change in the equilibrium than in the rate process, the net result of pressure being an increase in the over-all rate. At the temperature of maximum luminescence, the so-called optimum, the influence of pressure on the two reactions balances out.

Although the theory based on these two limiting reactions is oversimplified, it is sufficient to calculate with remarkable success the brightness of luminescence, in relative units, over most of the temperature range in which it can be measured, and for pressures up to some 10,000 pounds per square inch. The theoretical equation is

$$I = \frac{k'\,(\text{luciferin})\,(\text{luciferase})}{1 + K_1} = \frac{cTe^{-\frac{\Delta E^{\ddagger}}{RT}}\, e^{-\frac{p\,\Delta V^{\ddagger}}{RT}}}{1 + e^{-\frac{\Delta E}{RT}}\, e^{-\frac{p\,\Delta V}{RT}}\, e^{\frac{\Delta S}{R}}} \qquad (3)$$

in which I represents brightness of luminescence in arbitrary units, and c is a constant containing some known constants as well as others, e.g., the

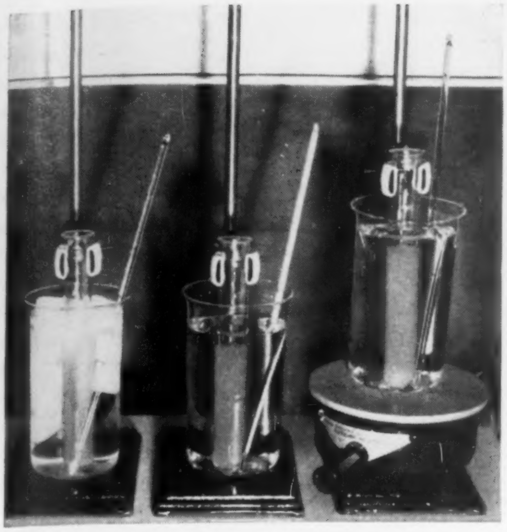


Fig. 4. Three tubes containing portions of the same suspension of luminous bacteria at (*left*) cold, (*middle*) optimum, and (*right*) unfavorably warm temperatures, respectively.

concentrations of luciferin and luciferase, which cannot be readily determined within a living cell. The other symbols have the usual meaning given above.

It is worthy of note that, first, equation (3) provides a rational basis for the interpretation of the "temperature-activity" curve; second, the reversible effects of hydrostatic pressure are accounted for on the same basis; and, third, this equation deals with the activity of a single enzyme system. Moreover, a number of chemical agents, including some of the familiar drugs, have been found to act upon the same system, introducing new reactions that limit its rate.

The action of chemical agents. From a study of the extracted and partially purified luciferin-luciferase system of *Cypridina,* it has been shown that the rate of light production may be slowed in the presence of drugs such as urethane, sulfanilamide, and others. Their inhibitory effects are removed by diluting the solution, and they evidently act by entering into an equilibrium combination with this system directly. In approximately the same respective concentrations, these drugs also inhibit the luminescence of living bacteria (Figs. 8, 9), likewise reversibly, since the inhibition disappears if the cells are centrifuged and resuspended in a drug-free medium. It appears safe to conclude, therefore, that the action in both the extracted system and in bacteria is fundamentally the same. Yet the mechanism of action of different drugs may be altogether different, as shown by the influence of temperature and hydrostatic pressure upon the amount of inhibition caused by a given concentration of the drug.

Qualitatively, the influence of temperature may be demonstrated by a simple experiment, as shown in Figure 10. A small amount of sulfanilamide solution is added to one tube (*left*), dilute alcohol to another (*right*), and pure salt solution is added to a third (*middle*) as a control for comparison. Equal portions of a bacterial suspension are added to each tube, and the luminescence of all three noted at different temperatures. In an ice bath (*A*) it is at once apparent that, although the alcohol causes no perceptible change in brightness, the sulfanilamide all but extinguishes it. As the three tubes are simultaneously warmed, all grow brighter until at the optimum temperature the sulfanilamide-containing suspension has increased in brightness manyfold, and is much more nearly equal to that of the control (*B*). The influence of alcohol, however, becomes increasingly manifest as the temperature rises, and it causes an obvious inhibition at the optimum temperature. At still higher temperatures, in a warm bath, the sulfanilamide tube remains luminous, whereas the one with alcohol is practically extinguished (*C*). All these effects are reversible on cooling; that is, the former relations obtain if the tubes are again placed in an ice bath.

The quantitative analysis of data from experiments of the type just described makes it evident that sulfanilamide combines in a manner that is independent of the denaturation equilibrium of the enzyme. It combines as if with some active group on the enzyme, involving chemical bonds that are not concerned in the thermal inactivation of the catalyst—perhaps with a prosthetic group. Conse-

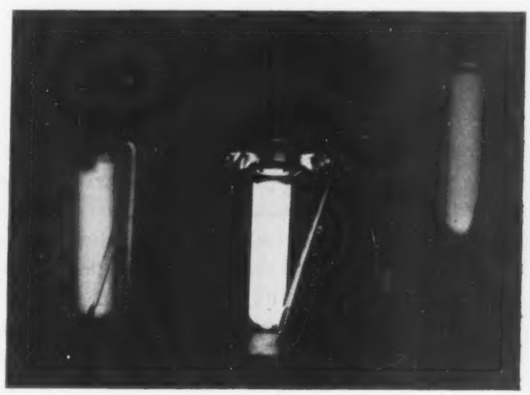


Fig. 5. Same tubes, as photographed by their own light, brightest at the optimum temperature, and dimmer when either too cold or too warm.

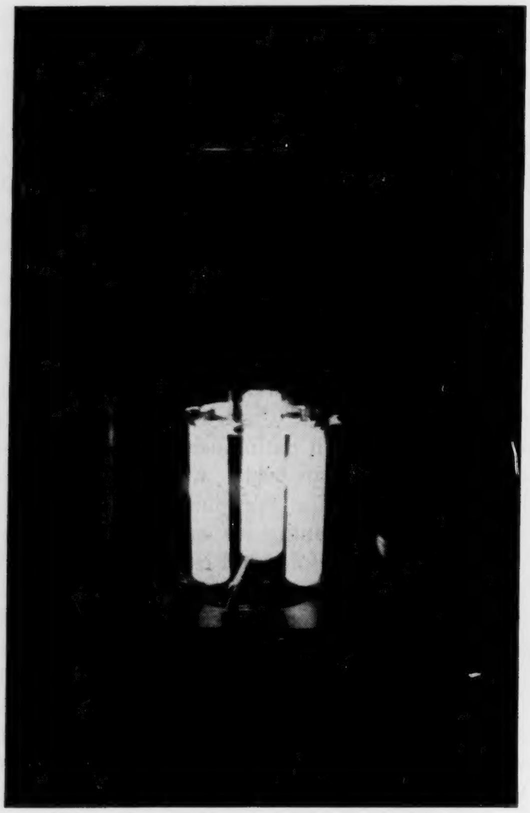


Fig. 6. Same tubes after placing all three in a water bath at optimum temperature. Both the heat- and the cold-diminution are reversible.

quently, as the temperature is raised, this combination becomes dissociated, and the percentage of inhibition is decreased. Alcohol, on the other hand, acts exactly as if it promoted the same equilibrium change from active to inactive states of the enzyme that is furthered by temperature in the absence of a drug; in other words, as if it catalyzed the denaturation of the protein. Or, viewed in another way, alcohol appears to combine with bonds on the protein that are made available through the denaturation. Thus, as the temperature is raised, the fraction of protein molecules with which alcohol may combine is increased, and the inhibition is augmented. In effect, the drug lowers the temperature of protein denaturation. A rise in temperature also tends to dissociate the protein-alcohol complex, however, and the net result depends upon the equilibrium constants for both these reactions. The heats of reaction are of opposite signs, and in the event that they are numerically the same the inhibition remains independent of temperature. Examples of this sort may be found in the literature with respect to other drugs and systems.

The influence of pressure on the action of these drugs might be anticipated from the above considerations. Combination with sulfanilamide evidently involves few bonds and leads to no pronounced change in the state of the protein catalyst itself. Consequently, pressure would be expected to have very little influence on the amount of the inhibition, since the volume change of the equilibrium would be small. On the other hand, because of its relation to the denaturation equilibrium which proceeds with quite a large volume change, the effects of alcohol might be expected to be sensitive to pressure, as, in fact, experiments have demonstrated. Thus, at the optimum temperature, where pressure ordinarily exerts little or no observable influence, the addition of alcohol causes an inhibition that, for moderate concentrations, can be completely eliminated by hydrostatic pressure.

The action of these drugs on luminescense, in relation to their concentration, to temperature and to hydrostatic pressure, is described with considerable accuracy by the following theoretical equation, in which K_1 represents, as previously, the equilibrium constant for the reversible denaturation of the enzyme in absence of drugs, K_2 the equilibrium constant for the combination of r molecules of sulfanilamide in molar concentration (X) with the enzyme, and K_3 the equilibrium constant for the combination of s molecules of alcohol in molar concentration (U) with the enzyme:

$$I = \frac{c\,T\,e^{-\frac{\Delta E^{\ddagger}}{RT}}\,e^{-\frac{p\,\Delta V^{\ddagger}}{RT}}}{1 + K_1 + K_2(X)^r + K_1K_2(X)^r + K_1K_3(U)^s} \qquad (4)$$

On dividing equation (3) by equation (4), the numerators on the right cancel out and simple formulations are arrived at for determining the equilibrium constants for the inhibitory reactions, provided that the inhibitors of the two types are not simultaneously present and entering into a possible combination with each other. The analyses indicate that one molecule of sulfanilamide combines with the enzyme molecule, whereas the ratio of alcohol molecules combining per protein molecule is from three to four. The heat of reaction in the sulfanilamide combination is around 15,000 calories, whereas that for alcohol is much higher and, unlike the former, is associated both with a large entropy and volume change.

Significance of the pressure effects. The foregoing discussion has dealt almost entirely with the process of luminescence as a type reaction, and

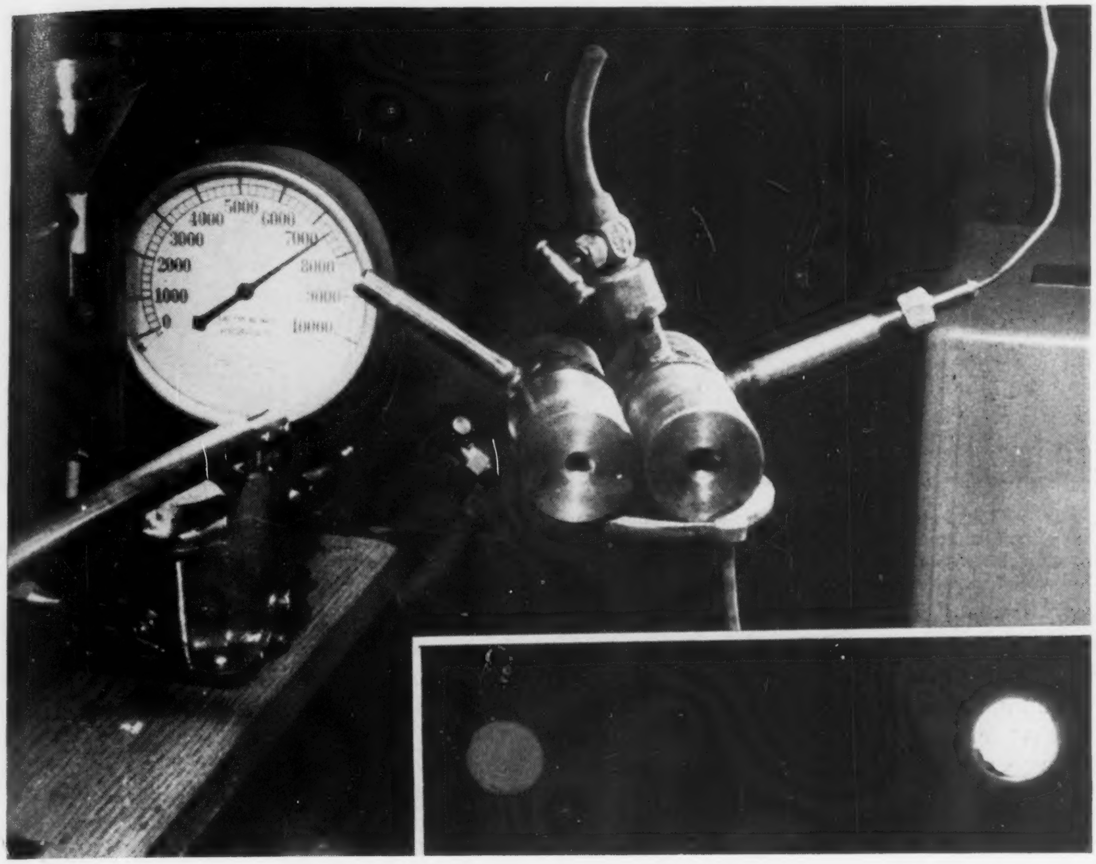


Fig. 7. Showing the influence of hydrostatic pressure on the brightness of bacterial luminescence. Two similar "bombs" are filled with equal portions of a bacterial suspension, both at a temperature high enough to cause a diminution in brightness. Bomb at right is then connected to the hydraulic press and the bacterial suspension placed under 7,400 lbs./in.^{2} pressure. *Inset:* Luminescence is visible through the plate glass window, .75 inch thick, in each bomb. Photographs show a close-up of the windows, taken by the light of the bacteria. The brightness of luminescence is much greater in bomb at right, showing that pressure counteracts unfavorable effects of the above-optimum temperature. A similar effect is noted at the optimum temperature if alcohol, urethane, or certain other drugs are first added to both suspensions. At cold temperatures, in the absence of drugs, effects are just the reverse; i. e., the window on right, showing the luminescence of the cells under pressure, would be dimmer than one on left, at normal pressure. In all these cases, the influence of pressure is in same direction as that of cooling.

the conclusions regarding certain mechanisms have been based very largely on kinetic evidence, i.e., the analysis of rate phenomena. Previous to the study of luminescence, the physiological effects of pressure, in particular, had not been interpreted on the basis of a rational, quantitative theory. There had been no evidence that hydrostatic pressure may retard the denaturation of proteins; on the contrary, a great deal of data had already accumulated showing that very high pressures, usually on the order of 10 times those which have been referred to, actually accelerate protein denaturation. Several questions, therefore, immediately present themselves. Is there, for example, any evidence more direct than purely kinetic evidence that protein denaturation can be retarded or reversed by pressure? Just what happens to a large molecule when it undergoes a volume change in a reaction? Why do moderate pressures, up to 1,000 atmospheres, retard protein denaturation, whereas very high pressures, of 10,000 atmospheres and above, accelerate it? Do the effects of the lesser pressures have any fairly general significance?

In answer to these questions, it should be pointed out first that, with regard to a rate process, kinetic evidence is not only the best but virtually the only kind that can be obtained for such volume changes of the molecules, and this is measured by the influence of pressure on the rate of reaction. This volume change takes place in the activated complex, which at room temperature

Fig. 8. The influence of drugs at the optimum temperature of luminescence in the species of bacteria concerned. Each flask contains equivalent portions of a bacterial suspension, to which has been added (*left to right*) small amounts of alcohol, aspirin, no drug (control), procaine, ether, and sulfanilamide, respectively.

has a lifetime usually of about 6 ten trillionths of a second, and only a fraction of the reactants are in the activated complex at any one time. With equilibrium reactions, on the other hand, it is possible to make direct measurements of volume changes between the initial and final state by means of a dilatometer. In this way it has been shown by Heymann that the denaturation of certain proteins—e.g., the coagulation of egg albumin by heat—is accompanied by volume increases of the same order as those revealed by the kinetic analysis of luminescence in bacteria. Furthermore, recent studies of the influence of pressure on the denaturation of highly purified human serum globulin and of tobacco mosaic virus, under the influence of heat as well as certain drugs, have shown that pressure retards the rate by a magnitude indicating about 100 cc/mole volume increase of activation. There is no doubt that in the cases investigated protein denaturation may be accompanied by a volume increase.

There is no basis in theory, however, for predicting that the volume change on denaturation will always be an increase, or that it will always be different in the final than the initial, or in the activated than in the normal, state. The volume change will depend upon the mechanism of the reaction, and, with molecules as complicated as

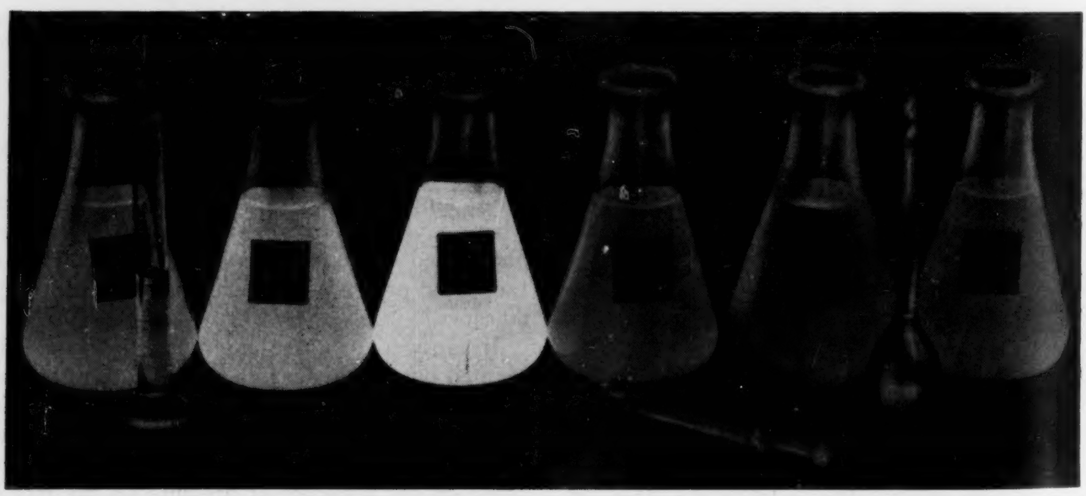
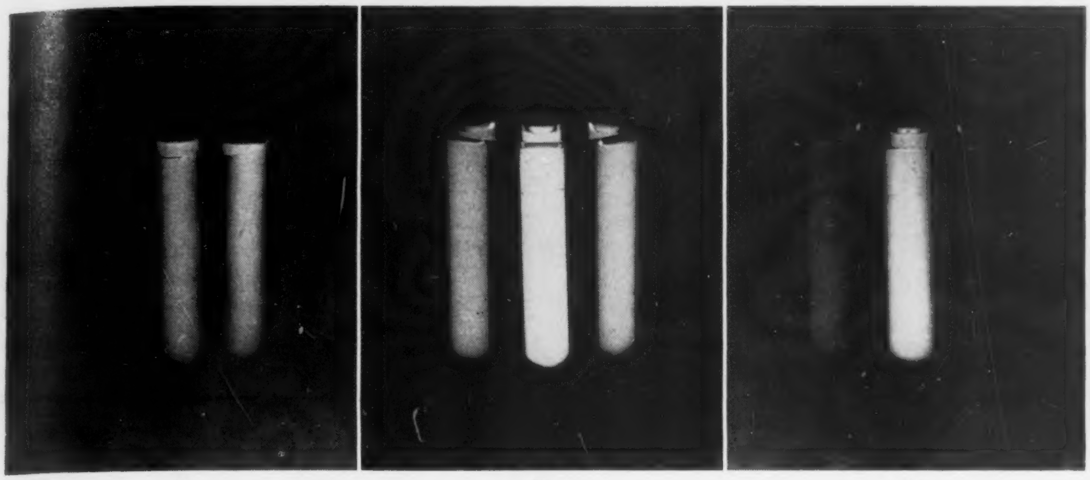


Fig. 9. Same flasks of bacteria, photographed by their own light, showing a partial (and reversible) inhibition of luminescent system through the action of the respective drugs.



A B C

FIG. 10. The relation between temperature and amount of inhibition of luminescence caused by two agents, sulfanilamide and alcohol, which act by different mechanisms. To three test tubes containing equivalent portions of a bacterial suspension, a small amount of sulfanilamide solution was added to the first tube (*left*), pure salt solution to the second (*control*), and alcohol to the third (*right*). Three photographs corresponding in all respects, such as time exposure, were then made by the light emitted from these tubes, when they were placed in a water bath first at a cold temperature (*A*), second at optimum temperature (*B*), finally at an above-optimum temperature (*C*). The relative effectiveness of sulfanilamide is decreased by a rise in temperature, whereas that of alcohol is increased. These effects of heating are largely reversible on cooling, as discussed in the text.

those of proteins, the highly ordered native structure may be altered through many pathways. The opposite effects of very high pressures at ordinary temperatures, and of moderate pressures at high temperatures, are an illustration of this fact. The mechanism of denaturation of proteins under very high pressures has not been clearly elucidated, although it has been suggested (Eyring) that it results from a change in structure of the solvent. It is known, for example, that there are at least five different forms of ice, of which only one (ice *I*, ordinary ice) is less dense than water. Under high pressure other forms of ice become stable, and such a change in the structure of the solvent might be sufficient to lead to instability of the complicated protein structure.

At high temperatures, the volume increase accompanying denaturation has been interpreted as a loosening of the secondary bonds which normally hold the chains of amino acids in a highly specific configuration. With the loosening of these bonds the whole molecule unfolds from a somewhat globular to a more fibrous form, and it is reasonable to expect volume increases of the order of magnitude that have been observed to characterize such denaturation reactions. Pressure opposes this unfolding by opposing the volume increase and, in equilibrium reactions, results in a refolding of some of the molecules. Thus, in a reversibly denatured state, at high temperatures, or in the presence of drugs such as alcohol, urethane, etc., the application of pressure causes bacterial luciferase to be virtually squeezed back together, with a restoration of its catalytic activity. Perhaps an analogous unfolding of the protein is necessary in the combination of certain enzymes with their substrates in the activated state. This would account for the fact that there is a fairly large volume increase of activation in the catalytic reaction, as in bacterial luminescence at low temperatures under conditions where essentially all the enzyme is normally in the native form. The activity of some enzymes, however—for example, yeast invertase—is apparently not accompanied by a significant volume change, suggesting that the active groups of such an enzyme are at the surface of the molecule, so that it does not have to unfold in the reaction. The active groups of the other type in the normal state are presumably folded up within the molecule.

Reversible physiological effects of moderate increases in hydrostatic pressure are widespread, as shown by studies on the contraction of muscle, the characteristics of the nerve impulse, amoeboid motion, sol-gel changes in myosin and other protoplasmic gels, specific precipitation of antibodies, and many other phenomena. The volume increases that have been interpreted as accompanying an unfolding of the protein, however, are of especial interest in connection with the theory of biosyn-

Photograph of the author, taken by the light of some 10,000,000,000,000 cells of *Photobacterium phosphoreum* luminescing in 12 liters of oxygenated salt solution.

bacteria, but that there are "barophilic" organisms, derived from the deep sea, that may grow as well (or better) under 600 atmospheres pressure as under 1 atmosphere, other things being equal. The nature of this adaptation to growth under pressure, as well as the whole relation of pressure to the metabolism of bacteria and other organisms in the ocean depths, offers an unusually challenging problem.

The quantitative theory for certain aspects of biological rate control that has been worked out in connection with luminescence has been extended, since 1942, to other processes. Some of them have already been referred to briefly in the above discussion of pressure effects. A few others are worthy of mention.

thesis and growth in general. For, in order to account for the synthesis of highly specific, optically active molecules, it seems necessary to postulate a templet mechanism, the first molecule acting as a pattern for the construction of a second one just like it. Moreover, in order for a native protein to act as a templet, it would seem equally necessary for it first to unfold into a one- or, at most, two-dimensional form. The evidence that has already been discussed leads one to expect that such unfolding will be accompanied by a volume increase of considerable magnitude. Pressure, therefore, should oppose the reproduction of proteins, genes, viruses, and other complex molecules in living cells. It has been recently shown to retard very markedly the rate of bacterial reproduction (*E. coli*). Current experiments (with C. E. ZoBell) indicate that this effect is of general applicability to pure cultures of numerous species of terrestrial

The relation between temperature and bacterial (*E. coli*) growth rate during the logarithmic phase at normal pressure is described with considerable accuracy by equation (3), from low temperatures to those that are high enough almost to prevent growth. The diminution in rate at high temperatures is immediately reversible on cooling, provided the cells are not maintained at such temperatures for more than a short time. Consequently, it appears that the rate of growth, like that of luminescence, is limited largely by the activity of a single enzyme system under the chosen conditions, and that the reversible denaturation of a single system again is the limiting mechanism at the above-optimum temperatures. Whether it is the same system concerned at both the low and high temperatures cannot be decided on the basis of the evidence at hand. Unlike luminescence, how-

ever, the rate of growth is not markedly increased by pressure at high temperatures, possibly for reasons (among others) that have been indicated in the discussion of volume changes that would be anticipated in connection with the biosynthesis of complex molecules. A pressure of 300–400 atmospheres retards growth at all temperatures in most ordinary bacteria and in all of several genera of yeasts that have been studied in pure culture, according to recent studies by ZoBell and Johnson. In some of these bacteria, however, growth will take place under pressure at high temperatures, where it apparently does not occur at normal pressure, in the same organisms whose reproduction is inhibited by the same pressure at lower temperatures. These results clearly represent the net effect of pressure and temperature on a number of possible limiting reactions, such as the catalytic process of enzymes, the reversible and irreversible denaturation of the proteins, synthesis, etc.

In contrast to the effects of slowing growth and reducing viability at ordinary temperatures, pressure aids in maintaining viability—i.e., slows disinfection—at higher temperatures. This is true even of bacterial spores at 95° C. Moreover, the destruction of spores at this temperature is accelerated by urethane in a manner that is opposed by pressure. In this case, also, the limiting reaction would appear to be one of protein denaturation.

The quantitative relation between inhibition of oxygen consumption and of methylene blue reduction in bacteria by urethane, both in relation to concentration of the drug and to temperature, is described with considerable accuracy by equation (4), as shown by the studies of Koffler, Wilson, and Johnson. Current studies in our laboratory have also shown that the thermal denaturation of tobacco mosaic virus is accelerated by urethane and may be opposed by pressure. The same is true of bacteriophage.

Although not all biological processes can be treated with the same simplicity as luminescence, as one would expect, the studies on luminescence have yielded a rational theory of much broader implications. They have anticipated results in diverse other processes. Numerous other phenomena, for which data adequate for analysis are not yet available, may be expected to disclose a quantitative interpretation on the basis of the same fundamental theory, appropriately extended to deal with more complicated situations. Thus, while the property of luminescence is of questionable advantage to many of the organisms possessing it, there is no doubt that it provides a unique and efficient tool to the investigator of basic mechanisms controlling biological reaction rates.

SCIENCE ON THE MARCH

PLANT PHYSIOLOGY AND RECENT PROGRESS IN AGRICULTURE

GREAT progress in agriculture often finds its origin in the plant physiological laboratory. Not so long ago manure was the only means the farmer had at his disposal for restoring to the land some of the fertility that cropping had removed. It was only after plant physiologists had learned to understand the fundamental principles of the mineral nutrition of plants that the chemical industry could make available effective fertilizers. Without modern chemical fertilizers large-scale farming would hardly be conceivable.

At the close of World War II plant physiology made another major contribution to agriculture: plant hormones. Today tons of these synthetic growth regulators are produced by the chemical industry, whereas before the war these substances could be found only in gram bottles on the shelves and worktables of research laboratories. These synthetic hormones are used as selective weed killers on crops such as sugar cane, corn, wheat, and barley; they are used in pineapple culture for the control of flower and fruit formation; in the apple industry to check the ruinous preharvest drop of fruit; and in the nursery for the propagation of plants by means of cuttings.

The introduction of synthetic plant hormones to agriculture has been so sudden, and the development of the field has been so fast, that the majority of people dealing with these compounds do not have the time to think of how all this originated. It is, of course, always difficult to find the exact starting point of any discovery. An idea grows slowly in the mind of several investigators, until suddenly one of them formulates it exceptionally well, or performs a critical experiment. With plant hormones, one could say that it all started when Charles Darwin, who was a capable plant physiologist in addition to being the famous student of evolution, studied the phenomenon of how plants orient themselves toward the light. In this study he used grass seedlings, and found that their tips were extraordinarily sensitive to light. An extremely accurate observer, he noticed that in order for a plant organ to curve toward the light it was not necessary for the entire organ to be exposed to the light. When the sensitive tip was exposed to unilateral light, the rest of the plant also curved toward the light source. This occurred even when the unexposed part of the plant was covered by black paper or hidden below the surface of the soil. In other words, Darwin found that the tip of the young plant had some way of communicating with the regions of the plant below it.

Darwin did not suggest a solution of this problem, but it aroused interest in academic circles, and, under the leadership of P. Boysen Jensen, of the University of Copenhagen, the problem was taken under investigation. Gradually it became more and more apparent that a chemical compound naturally occurring inside the young plant served as the messenger which told one part of the plant what was going on in another. At the University of Utrecht, nearly fifty years after Darwin's experiments, it was conclusively proved that a chemical was involved by F. W. Went, who in 1926 separated the active principle from the plant.

Even though proof now existed that different parts of a plant communicate with each other by means of a chemical principle, the nature of the compound still remained obscure. However, by 1934 this problem was also solved. At the University of Utrecht, Kögl and Haagen-Smit identified indoleacetic acid as one of the compounds involved. Since this substance could be synthesized cheaply, it was readily available for experimentation.

About this time plant hormone research in the United States began at the California Institute of Technology, eventually spreading to other laboratories in the country. Soon it was learned that applications of indoleacetic acid to plants will prevent development of dormant buds in the axils of leaves, that indoleacetic acid will promote root formation on cuttings, and that it will cause fruits to set without the benefit of pollination.

In the meantime, homologues and analogues of indoleacetic acid were developed—principally at the Boyce Thompson Institute for Plant Research—which served practical ends better than the naturally occurring indoleacetic acid. Among these new synthetic plant hormones was one—a chlorinated phenoxyacetic acid—which was extremely persistent in the plant and at the same time highly active. This compound, now known as 2,4-D, is by far the most widely used of all synthetic hormones. In low concentrations it could be termed a stimulant, in

that it causes pineapple plants to flower, promotes root formation in a variety of plants, and prevents citrus fruit and apples from falling from the tree before harvesttime. In higher concentrations, however, 2,4-D turned out to be the most effective selective plant exterminator ever devised. It destroys the growing point of many broad-leaved weeds, thereby effectively preventing their recovery. Because it is relatively harmless to plants of the grass family, it is widely used on such economically important crops as sugar cane and the cereal grains, all of which are technically grasses.

This great effectiveness, coupled with the relatively low price at which the chemical industry made the material available, has changed the whole aspect of weed control. It can be said that, as a result of the impetus given to the problem of weed control by the discovery of 2,4-D, more progress has been made in this field during the past five years than in the entire previous history of agriculture.

Striking though this application of plant hormones may be, it is by no means all that has been achieved from a practical point of view. In Hawaii, synthetic plant hormones are being used on a large scale to force pineapple plants into flower. Another hormone is used to regulate the vegetative propagation of this plant, and still another synthetic hormone is used to increase the fruit size and step up the sugar content. To make the picture of crop control even more complete, synthetic hormones are also used to prevent pineapple plants from going prematurely into flower and fruit production.

Looking back upon their work, plant physiologists can be gratified with the contribution their research has made to agriculture. Looking ahead, they, and those supporting their work, may be confident that fundamental research will continue to uncover the ways in which plants function, and by applying this knowledge contribute further to progress in agriculture.

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IDENTIFICATION OF THE BEST SOUTHERN NEGRO HIGH-SCHOOL SENIORS

THAT there are in some population groups a "normal" proportion of Negro subjects of very superior psychometric intelligence was shown in a recent article in The Scientific Monthly (May 1948) by Martin D. Jenkins, formerly professor of educational psychology at Howard University and now president of Morgan State College. He points out, however, that the populations producing these superior deviates are located almost exclusively in the Northern urban communities, and he adds: "Whether the fact that no children [in Southern states] with this development have been discovered is due to lack of environmental opportunity and stimulation, or merely to lack of identification, is not surely known."

Believing that Negro high-school seniors of superior intelligence do exist in each of the Southern states having segregated school systems and that these able students should be identified and their further education be made possible, the group of educators who make up the Pepsi-Cola Scholarship Board have developed a special program for Negroes in the South. This Board annually awards at least 119 four-year college scholarships and 550 college entrance awards to white and Negro students in the United States and territories. In planning the Southern phase of the program, it has been recognized that the environment of these students is inferior; that their schools, on the average, are substandard; and that these and other factors will depress the psychometric intelligence of such pupils.

The practical problems encountered in attempting to identify the most able Negro high-school seniors in the Southern states are many and varied. In the first place, the cooperation of the schools must be obtained for preliminary identi ation in the Pepsi-Cola Scholarship program. It a school refuses to enter the program for any of several reasons, able students may be overlooked. Although no fees, premiums, or pledges are required of the school or of the participants, there is a certain amount of suspicion among educators of any scholarship program financed by a commercial concern. (Actually, the Pepsi-Cola Scholarship Board is an independent corporation directed and completely managed by educators. The Pepsi-Cola Company, at the request of its president, Walter S. Mack, Jr., finances the project.) Also, many Negro school officials believe that no pupils of theirs have any possible chance of continuing their education on the college level and that participation is, therefore, pointless. Teachers in industrial types of high schools often fall into this category.

Second, since the preliminary test must be given in each participating school, able students may be at a disadvantage and less able ones aided if the test is not administered correctly.

With these problems in mind, the Board has set up this method of identification: Once a school has registered, candidates for the scholarships are chosen by a senior class election on the basis that they are the ones "most likely to make an important contribution to human progress." Any school, no matter how small the senior class, may have 2 representatives. In the larger schools, up to 5 percent of the senior class may be entered.

These candidates then take the preliminary selection test—prepared and scored especially for this program by the College Entrance Examination Board—in their own schools and administered by a member of the school staff. Approximately 8 of the highest-scoring pupils in each state on this first test then are asked to take the supervised test of the College Entrance Examination Board at its regular series. The Scholarship Board pays the fee required for this test. On the basis of this second test, the highest-scoring Negro student in the state, regardless of the level of his score, receives a four-year college scholarship. The scholarships pay full tuition and fees for the college of the winner's choice, a travel allowance, and $25 a month for thirty-six months of college. The next several in order, up to 5 percent of those participating or a maximum of 5 students in any one state, receive College Entrance Awards, which pay $50 upon the winner's entrance into college. Even low-scoring contestants, if they are the best in the state, receive these awards. In this way, it is believed that the faith of high-school officials in the program can be established, increasing participation, and thus making improved selection possible.

That this method of selection, to a degree at least, is effective is borne out by the results. In spite of all the practical difficulties, participation has increased from 561 Negro seniors, representing 179 segregated schools in 1945, to 1,875 students from 652 schools in 1948. Participation this year included 40 percent of the 1,600 Negro secondary schools in the 17 Southern states and the District of Columbia. A total of 28,140 seniors took part in the class election—or approximately 73 percent of the Negro seniors in the South's segregated schools. According to the 1945–46 figures of the U. S. Office of Education—the latest obtainable—there were 38,466 seniors in the segregated schools of the South.

By having the Negro finalists on the preliminary test given in the schools take the supervised test of the College Board for the final selection, it is possible to make a comparison of their scores with those of white students throughout the country taking the same test. The College Board Scholastic Aptitude Test, which is given only at supervised centers, is required of applicants to many of the Eastern institutions and is taken by what is probably the most highly selected group of comparable size seeking admission to college.

This Aptitude Test yields two scores, one on the verbal factor and one on mathematics. For the purposes of scholarship selection in the Pepsi-Cola competition this year, a final score was made by adding to the score on the verbal section one fifth of the score made on the mathematics section.

Table 1 lists the scores of the 18 winners of four-year college scholarships on both the verbal and mathematics sections of the Scholastic Aptitude Test of the College Board. The scores are reported on a scale ranging from 200 to 800 with a mean, or average, of 500 and a standard deviation of 100. One of the Negro winners scored 690 on the verbal section and 592 on the mathematics. A score of 690 ranks at about the 97th percentile of the College Board norms. This Negro winner from South Carolina is undoubtedly of superior intellect. Nine additional Negro winners scored above the average of this highly selected white college entrance population; 3 colored winners of the scholarships scored below 400 on the verbal section, thus falling into the lowest 16 percent of the College Board group.

TABLE 1

SCORES MADE BY NEGRO FINALISTS, 1948 PEPSI-COLA SCHOLARSHIP PROGRAM, ON THE COLLEGE BOARD SCHOLASTIC APTITUDE TEST

Reported Score*	Number of Finalists Winning							
	Four-Year Scholarships		College Entrance Awards ($50)		No Awards		Total	
	V	M	V	M	V	M	V	M†
600–699	1	..	..	1	..	..	1	1
500–599	9	3	3	6	..	1	12	10
400–499	5	8	18	24	2	9	25	41
300–399	3	7	41	38	19	27	63	72
200–299	..	..	18	11	25	9	43	20
Total	18	18	80	80	46	46	144	144
Average Score	488	436	355	387	300	354	354	382

* The average score on the College Board norms is 500, and approximately two thirds of its group score between 400 and 600.

† V is the verbal section; M, the mathematics section.

For comparison, the 130 white winners of scholarships averaged 762 on this same test, and the 507 College Entrance Award winners (or runners-up) among the white group averaged 696.

This program has been a successful method for identifying superior Negro college material. In the past three years, 59 Negro high-school seniors have been awarded the four-year college scholarships. Although there has been some mortality, 4 of these 59 having had their scholarships canceled, some of these students are now succeeding under current competitive conditions at each of the following colleges and universities: Columbia, Illinois Institute of Technology, Northwestern, Oberlin, Radcliffe, and Western Reserve, as well as at Fisk, Howard, Tuskegee, and a number of other institutions.

There still remain many practical problems in locating Negro high-school seniors of superior scholastic aptitude in the Southern states, but there is no reason to believe that there is a low upper limit of intellectual ability among this group. In the light of results achieved after four years, the Pepsi-Cola Scholarship Board believes that progress is being made.

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COLUMBIA UNIVERSITY RESEARCH IN CONTEMPORARY CULTURES

A VERY common question with which the intelligent layman greets the anthropologist just returning from a field trip is "What were the people *like*?" The question is a legitimate one, though extremely difficult to answer, and it concerns a scientific problem which, curiously, has been one of the last to be taken up by anthropologists. Usually the anthropologist will attempt to answer the question by describing various facets of the tribe's culture, only to be asked insistently, "Yes, I know. But what are the *people* like?" Anyone who travels, whatever his scientific or other purposes, is of course aware of significant differences in national character; indeed, the people of various countries are well equipped with stereotypes concerning foreigners of different nationalities. These stereotypes, however, from the point of view of the anthropologist, are more an aspect of the culture of the people holding them than they are verified scientific knowledge.

Anthropologists traditionally have been so concerned with preserving some record of fast-disappearing preliterate cultures that one is apt to forget that there is no reason why their well-tested field methods should not be applied to the study of contemporary literate cultures. Great as the theoretical significance and scientific interest of such studies would be, these considerations are far outweighed by the very great practical importance of such knowledge. International relations are constantly impeded by inexplicit, unconscious differences of assumption and cultural expectation; and certainly a major (and irrational) cause of wars is just these cultural incommensurabilities that are inaccessible in most cases to conscious reasoning and judgment. Every successful diplomat has as his chief duty "understanding" the people of the nation to which he has been assigned, and he must acquire a sizable body of experience and information which will enable him to assess, to judge, and to predict the behavior of this people. But his knowledge is often unformulated or inarticulate and usually lost to others scientifically interested; the proved methods of field ethnography might gather much the same kind of data more efficiently, voluminously, and systematically. In peacetime such an understanding of the predictable regularities of national character would tend to preserve world peace; in wartime the military uses of such knowledge for "white" and "black" propaganda, for strategic planning, and for actual military operations are of almost incalculable importance. A notable instance in World War II was our failure to understand and to predict Japanese diplomatic and military behavior in terms of their character structure—when the data, indeed, are quite accessible to a scientific approach.

Whereas most studies of national character structure have suffered from an impressionism inevitable from an unsystematic gathering of relevant data, and from improperly weighted judgments based on too-few instances of the phenomenon, the Columbia University project for Research in Contemporary Cultures is notable for its systematic approach, voluminousness of data, and methodological rigor. Not only are the formal categories of field ethnography used, including extensive and repeated interviewing, but in addition the checks and tests of psychology and the insights of psychiatry are also used. These include test materials (the Story Test, new forms of the Horn-Hellersberg Test, the Thematic Apperception Test, and the Rorschach Test), as well as analyses of novels, cartoons, and motion pictures produced by representatives of the nation concerned. Full verbatim interviews are obtained and kept on permanent file for later reference; of these a keyed abstract is